

The Scramble for Petroleum

PRESIDENT NIXON'S long-awaited message on energy, sent to Congress last week, appears to be principally a holding operation. It is certainly not the radical reappraisal of the American energy economy which had been looked for. The immediate need, apparent for the past three years, is to increase the availability of petroleum and its products. Plainly it will help with immediate problems that the import quota system on imported petroleum should be replaced by a complicated system of tariffs, the full economic effects of which will not be felt until 1975. If American importers can find the ships and lay their hands on supplies of crude oil from the Middle East, they will at least be able to ensure that refineries in the United States operate at full capacity in the months ahead. And two or three years from now, American competition for Middle East oil will be more substantial—the proposed levy of 21 cents on each barrel of imported crude oil is unlikely to cut back the import programme to a large extent, especially because the steps which are intended to stimulate domestic petroleum production are unlikely to yield their principal benefits quickly. What this implies is that the world as a whole must reconcile itself to increased competition from the United States on the world petroleum market, a development most likely to be welcomed by the OPEC countries. The policy of "Drain America First" will bring results only in the later years of this decade and in the 1980s.

It goes without saying that the steps which President Nixon has now taken to increase production of petroleum in the United States are sensible. It is, of course, anomalous that the country which consumes the lion's share of the world's production of petroleum should, in the past decade, have followed policies which only exacerbate its own predicament. The notion that the Federal Power Commission should in future be less zealous in restricting the price at which natural gas is sold is especially important—in the past few years, the pegging of prices at uneconomic levels has done more harm than physical shortages. On offshore drilling and the building of the Alaska pipeline, the President has been forced, rather late in the day, to resolve the conflict which has grown up between environmental considerations and the rising demand for petroleum in the United States—in many ways he could have said where the federal government stands much earlier. The Alaskan pipeline has been dealt with especially timorously—it has been plain for the best part of two years that legal battles in the courts could only create such confusion and delay that new legislation would be necessary. It is inexcusable that the Administration should have shillyshallied for the past year when the prospect of a petroleum shortage was steadily worsening.

And even now the Administration has not faced up to the underlying problem of petroleum in the United States—that most petroleum products are at present sold at prices which are uneconomically low and which

offer few of the incentives to more economical use which the circumstances require. Gasoline prices in the United States are only a half of those now current in Western Europe. Both natural gas and heating oil are comparatively cheap, with the results that incentives for thermal efficiency are diminished. Yet there is an obvious need that the pace of consumption of petroleum in the United States should be brought much more closely into line with the pace at which supplies are likely to increase in the next few years, which implies that Americans should be required to think twice about the ways in which they consume gasoline in particular and petroleum products of all kinds as well. To be sure, the consequences would be considerable—the gay abandon with which motor cars are at present used would be diminished. But that is one of the realities of life with which the United States must sooner or later come to terms. The sooner the better, if demand is to be sensibly matched against supply.

Fun and Games with GNP

ONE of the strange manifestations of the environmental disputes of the past few years is the way in which the Gross National Product has come in for criticism and unwarrantable reinterpretation. Some years ago, Professor Kenneth Boulding, in a book review with the same title as this leading article, quite legitimately pointed out that the Gross National Product, which is a perfectly proper statistic to use as a measure of a nation's economic activity, is not necessarily a measure of a nation's well-being or of the happiness of its people. From this, of course, no classical economist will dissent. One of Boulding's own illustrations of the point is the case of a man who employs a housekeeper whom he later marries. To begin with, he pays her a salary which is added to the GNP. But the housekeeping money which she later receives does not, by convention, count. In other words, the transition to the married state is accompanied by a reduction of the GNP, but who would not agree that the sum of human happiness has most probably been increased? Classical economists and others would also, of course, agree that while the GNP may be a useful measure of the comparative economic activities of two very similar countries, Britain and France for example, there is not much value in using it as a yardstick for comparison between countries as dissimilar as, say, Britain and India. The value of the statistic to the economists is that it establishes an algebraic identity between two different sets of variables—the total value of goods and services produced in any year (imports and exports assumed negligible) and the sum of personal expenditure, public expenditure and saving. It is no surprise that the



Gross National Product has been and will continue to be a necessary tool in the formulation of policies intended to relate, for example, a government's budget deficit to the rate of industrial output.

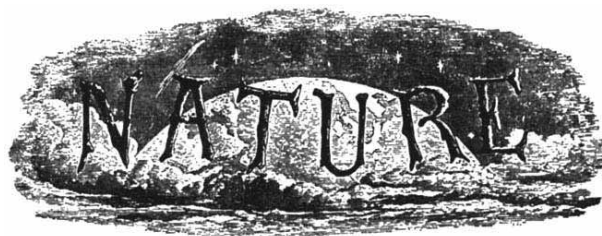
From this point of view, the crusade of the environmentalists against the Gross National Product is as absurd as would have been a crusade against the Gibbs Free Energy on the grounds that it does not provide a simple rule for showing how energy will flow from one system to another except when the characteristics of the second system are defined. To be sure, there are now, as there always have been, pertinent questions to ask about the kinds of economic activities most likely to benefit a particular community, and it does not always follow that the changes most likely to lead to the rapid growth of GNP will be the most desirable or the most desired. It remains something of a puzzle to know why the critics of the GNP as a measure of human happiness should be so anxious to define other yardsticks that might be more suitable. This, at least, is now a popular sport. No doubt it will be with us for a long time to come.

The article by Mr R. E. Overbury on page 561 of this issue of *Nature*, which has been published at his request against the advice of qualified referees and without revision in the light of constructive editorial suggestions, is a typical example of the new genre. The interest of Mr Overbury's article, and the point which needs developing, is the usefulness of matrix methods (with which economists have long since been familiar) for the analysis of how different components of the economic activity of a country can be related to the input of primary material. The essence of his error is the curiously subjective way in which the Gross National Product itself is redefined as the sum of gross consumption—equated with the "real cost", whatever that may be, to society—and the rest, supposed to be used in "improving the state of society". By any test, this is a thoroughly subjective definition. Mr Overbury himself admits that his distinction between good and bad is somewhat arbitrary. He could have gone further and said that it is nonsensical. As far as can be told, his objective is to distinguish those activities which consume resources and which are therefore to be discouraged in a closed society and those which improve the state of society and which are assumed, for no particular reason, to be compatible with a closed environment. But there are obvious flaws in this argument. Education, for example, supposedly in Mr Overbury's sense one of the economic activities that lead to the improvement of society, is a considerable consumer of resources in exactly the same way as other less well favoured kinds of economic activities. For even if teachers who stand in front of classrooms make only modest demands on resources, the structures in which they teach are a drain on raw materials while the salaries which they are paid, like the salaries which go to the growing (but still inadequate) armies of ancillary workers in education, are eventually spent on exactly the kind of consumption which Mr Overbury labels bad. In short, his arguments for a reinterpretation of Gross National Product are both unnecessary and misleading.

A second flaw in the argument that Mr Overbury presents is the implicit assumption that the time has come to regard the economic system of the world as a whole as a closed system. This is manifestly untrue. For one thing, a great deal of agricultural production is a direct

consequence of the conversion of solar energy into edible chemicals. For another, even though it is in some literal sense true that the Earth is finite, the extraction of raw materials is at present based on such a limited technology that it consists of almost literally scratching the surface of the Earth. For example, although uranium mining at present entails essentially conventional mining operations, the time may yet come when extraction from seawater will enormously increase this natural resource. To point this out does not imply that a consideration of closed economic systems has no value, but, like all essentially academic exercises, its object should be to identify those parts of the problem likely to have a bearing on the real world. To claim more, as Mr Overbury has done, is to confuse.

100 Years Ago



East India Museum

ALLOW me to make yet another suggestion (in addition to those of P.L.S. and Prof. Newton), with regard to the disposal of the natural history collections at the India House. It seems to me to be one of the greatest popular delusions, that specimens of natural history necessarily require lofty halls and spacious galleries for their preservation and exhibition in a useful manner. I hold, on the contrary, that, with few exceptions, they far better serve educational and scientific purposes when arranged in ordinary apartments. All the scientific work in the British Museum is done in small rooms; and the palatial galleries with their crowded myriads of specimens and miles of glass cases, however instructive they may be (or might be made) to the public, are a positive hindrance to scientific work. I am very much mistaken if all the India House natural history collections might not be suitably placed in two or three ordinary sitting rooms, and so arranged in cabinets and boxes as to be far more convenient for reference and study than they have ever been. The rent of a moderate-sized house in an airy situation, say 250*l.* with an equal sum for the salary of an efficient Curator, and a small grant for cabinets and the necessary books of reference, is all the expense required to make this interesting collection completely accessible to all who wish to consult it. Every one interested in Indian natural history would then visit it. It would again receive gifts of collections from travellers, Indian Officers, and other persons interested in the natural history of the East; and its increase in value from this source alone might go far towards furnishing a tangible equivalent for the expense incurred, while it would certainly render the collection a better representation of the Indian fauna than it is at present, and more worthy of a place, at some future time, in the proposed grand Indian Museum.

Such a modest establishment would also, I believe, do much good by showing at how small an expense a really useful scientific museum may be kept up, and would thus encourage the formation of local museums in cases where 20,000*l.* or 30,000*l.* cannot be raised for a building. It would not, of course, be a show museum for the uneducated public to wander and gaze in;—the British Museum serves that purpose. But it would prove greatly superior to any such mere exhibition, as a means of furnishing definite information on Indian zoology, and enabling any intelligent inquirer to obtain some idea of the many wonderful and beautiful forms of life which characterise what is at once the smallest and the richest in proportion to its extent, of the great zoological regions of the globe.

ALFRED R. WALLACE

From Nature, 8, 5, May 1, 1873.

OLD WORLD

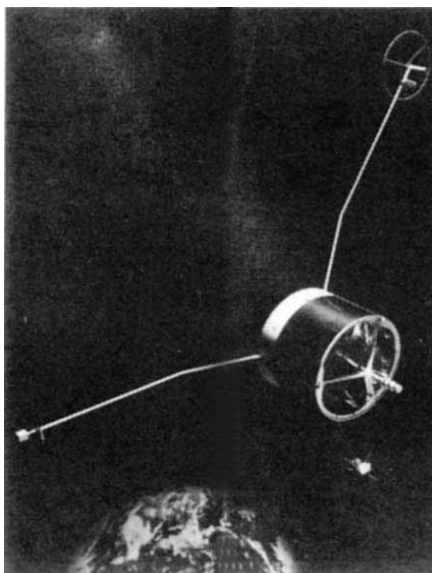
ESRO Chooses Two New Scientific Satellites

THE European Space Research Organization has decided to fly two new scientific satellites in 1977 and 1979. The final choice has fallen on HELOS (Highly Eccentric Lunar Occultation Satellite) which will examine X-ray sources, and ISEPS (International Sun-Earth Physics Satellites Programme) one of a pair of satellites designed to study the earth's magnetosphere and the adjacent boundaries of interplanetary space. The second satellite of the pair will be launched by NASA.

The decision to launch these two satellites commits ESRO to an expenditure of 63 million units of account (about £19 million) on HELOS and 23.4 million units of account (about £7 million) on ISEPS, which is also known as the mother-daughter satellite.

It was originally thought that only one out of the three projects up for consideration by the ESRO council would be chosen (see *Nature*, 242, 79, 1973). The unsuccessful project was the Venus orbiter, designed to investigate the atmosphere and surface of Venus, and this has gone by the board chiefly because NASA, which was to launch the satellite and provide some of the experiments, has no provision in its 1973-74 budget for the satellite, and ESRO was not prepared to go ahead alone, even though the council was satisfied that the project was scientifically sound. Without NASA's participation the need to buy a launcher and provide all the experiments and ground tracking facilities would have raised the cost considerably for ESRO. Nonetheless the project will probably be represented when ESRO chooses its next set of satellites in 1976. There is another launch window in 1980 (originally it was hoped to launch in 1978) and if NASA is interested the project may go ahead then.

Meanwhile cooperative plans go ahead to launch the mother-daughter satellites in 1977. This project, which consists of two satellites launched on the same Thor-Delta rocket, will be tied in with NASA's heliocentric satellite project to provide detailed information about the way in which the solar wind affects the magnetosphere of the Earth. The heliocentric satellite will monitor the solar wind, while the two ESRO satellites, in different orbits in the magnetosphere, report changes in the radiation belts, the magnetopause (the boundary between the Earth's magnetosphere and the solar wind flowing past) and the bow shock and tail produced by the movement of the magnetic Earth



The daughter half of the joint ESRO and NASA mother-daughter project.

through the charged solar wind.

The HELOS project, although entirely independent of any other project, provides the logical and much needed next step in the development of X-ray astronomy. Because of its highly eccentric orbit, the satellite will "see" very many sources occulted by the Moon. Accurate timing of the occultations provides the most accurate measurements of the positions of these sources that is possible from X-ray observations alone. These in turn should lead to identifications with optical and radio sources—at present more than 200 discrete X-ray sources are known but only a handful have been identified with known astronomical objects. Hopefully, this sort of identification will help to resolve some of the problems of X-ray stars (are they associated with black holes, for example). The satellite also has the ability to study variable sources for several hours at a time from above the interference of the radiation belt.

EUROPE

Needed Collaboration

SLOW but sure progress is being made towards setting up a European Science Foundation. In Munich on April 13 and 14 representatives of national science organizations in ten countries in western Europe — Britain, France, Austria, Ireland, Italy, Netherlands, Spain, Sweden, Switzerland, and Germany—met for the second time in five months to discuss the possibilities and difficulties of such a venture.

The first meeting was held at the Royal Society last December when the European Economic Commission's proposals for a common scientific, technological and industrial policy, published as *Objectives and Instruments of a Common Policy for Scientific Research and Technological Development*, was discussed. But the conference, last December, felt strongly that any common European Science Foundation should be a western European organization and not restricted to countries within the EEC. A similar feeling was prevalent at Munich.

But still no decision has been made as to what form European cooperation in fundamental research should take in the coming years. Yet another meeting is planned to consider the fields in which fundamental research should be intensified and what form of organization might be suitable. This meeting will take place in Paris at the end of Sep-

tember. In the meantime, however, the representatives of the national research councils and academies who attended the Munich meeting are considering the most appropriate form for the foundation. In particular, before such a foundation is set up, the relationship of the foundation to the already existent European collaborative projects must be determined. For example, the medical research councils of Europe already meet regularly and the European Scientific Exchange programme ensures contact between the Royal Society and other academies.

But how will the European Scientific Foundation be financed if agreement is reached next September on setting it up? Dr R. W. J. Keay, of the Royal Society said this week that it is hoped that the research councils and the academies will contribute part of their budgets towards the foundation but that some support is expected from the European Commission as well.

Even if no decisions emerged from the Munich meeting it at least showed that the thinking within the European Commission is not too divergent from that of European scientists. The fear expressed at the meeting in London in December was that the EEC would impose a European Science Foundation on European scientists possibly against their wills. But the presence of Professor R. Dahrendorf, Commissioner with responsibility for Science and Technology, at Munich, showed that a common objective can be achieved.

ANGLO-SOVIET SCIENCE

Hope for the Future?

A CEMENTING of relations between Britain and the Soviet Union in the applied scientific, technological, trade and economic fields took place in Moscow last week.

This was at the second meeting of the permanent United Kingdom/USSR inter-governmental commission (Joint Commission) for cooperation in these fields, the first of which was held in January 1971. After that meeting, enthusiasm for the aims of the Joint Commission waned, but last October a ten day visit to Britain by a delegation from the Soviet State Committee for Science and Technology put much needed life into Soviet-British relations in science and technology.

The British delegation to Moscow was led by Mr Peter Walker, Secretary of State for Trade and Industry, while the Soviet group at the meeting was led by Academician V. A. Kirillin.

The Joint British/Soviet Commission called for a greater cooperation between scientific research organizations and companies in the two nations.

But the *communiqué* released from Moscow after the meetings last week is more a statement of intent than a plan for action. It is clear that both sides expressed willingness to cooperate and exchange ideas, but the driving force to get cooperative ventures off the ground must come from the scientists and from industry. The *communiqué* makes special mention of collaboration that already exists—for example, in plasma physics, where scientists from the Culham Laboratory of the United Kingdom Atomic Energy Authority are collaborating with their Soviet counterparts and also in radio astronomy.

The new lease of life given to the Joint Commission by last week's meeting means that both Britain and the United States now have an effective machinery for cooperation with the Soviet Union. An agreement was signed between the United States and the Soviet Union in May 1972 effectively setting up a committee very similar to the one that met last week (see *Nature*, 237, 247; 1972).

But how does cooperation work from the British side? Before the Joint Commission was set up in January 1971 there were in existence several working groups on specific technological and scientific problems which were set up in 1967–68 under the auspices of the Confederation of British Industry and the Soviet State Committee. One of the articles of the agreement which set up these committees stated that there should be exchange visits between applied scientists and technologists in

Britain and the Soviet Union. This was designed to complement the already existing, and active, exchange programme between the Royal Society and the Soviet Academy, which chiefly caters for pure scientists. But from the British point of view the exchanges under the 1967–68 scheme have not been completely successful. Only a few exchanges have taken place and the British people involved have generally had to pay their own way to Moscow, although during their stay they are sponsored by colleagues in the Soviet Union. The few Soviet scientists to come to Britain have done so on a similar basis.

It is, however, cheerful to note that the next meeting of the Joint Commission has already been set for London—sometime in 1974.

EDUCATION

Sir Brian Defends

SIR BRIAN FLOWERS, chairman of the Science Research Council, said last week that his council “regarded it as a matter of national importance to further the engineering profession”, as little has been done in the training of engineers during the past 20 years. Giving evidence to the Select Committee on Finance in Education and Arts, Sir Brian said that the SRC has attempted to rectify the situation by using “its most powerful weapon—its money”.

The universities pay for about half the research studentships in the sciences. The SRC's policy of allocating more grants to applied research was, at first, hindered by the universities allocating more grants to the pure sciences. Now, according to Sir Brian, the situation has changed and considerably more emphasis is being placed on the applied sciences.

This bias towards applied science raised the question as to whether a separate Technical Research Council should be formed—possibly as a branch of the SRC. Sir Brian insisted that “both pure and applied science would suffer” if the SRC was subdivided.

Little information is available about employment of higher degree graduates, but both the select committee and the SRC agreed that better liaison between universities and industry is necessary to ensure that worthwhile research projects are financed.

Sir Brian hotly defended the awarding of fellowship grants, and he spurned the suggestion that these fellowships were just another form of unemployment, saying that they were “a subsidy to real and lasting excellence”. He pointed out that postdoctoral grants are only awarded to a very small proportion of PhDs and their duration is for two years only. Their purpose is to

allow a brilliant and original researcher to carry on his project unhindered by any outside pressure.

INDIAN SCIENCE

Concern over Quality

from a Correspondent

UNEMPLOYMENT among scientists is as much, if not more, of a problem in India than it is in Britain and North America. There were five times more scientists unemployed in India in 1972 than there were in 1967, whereas unemployed arts and humanities graduates were 3.5 times more numerous in 1972 than they were five years earlier. But twice as many degrees are awarded in the arts than are awarded in the sciences each year.

In 1966–67, 45.5 per cent of the students who enrolled at Indian universities did so to take degrees in scientific and technological subjects. But the percentage has dropped every year since then and in 1970–71 it was as low as 41.4 per cent of the total enrolment.

This is symptomatic of the state of science in India, which has been in the doldrums in recent years in spite of a massive increase in support for scientific research from R1,300 million in 1967–68 to R2,140 million in 1971–72. There is deep concern about this, although ambitious plans are being formulated by scientific groups within India to use science and technology in order to eradicate poverty. At present several Indian scientists hold the rank of secretaries to the government and a phase of evaluation and consolidation in science is taking place.

But there is widespread concern about the quality of the publications of Indian scientists. This concern is not lifted by the results of a survey, carried out by Professor Rais Ahmed of the Indian University Grants Commission on publications of scientists working in India in the fields of spectroscopy, nuclear physics, electronics and solid state physics. The survey shows that in 1969, for example, 181 Indian scientists were cited in physics abstracts journals as having produced papers in spectroscopy, but in 1971 only 95 were cited. But in the same period the number of names of foreign scientists working in India who had published papers in spectroscopy increased from 100 to 162. A similar decrease in the numbers of publications by Indian born scientists and increase by foreign scientists was recorded in most of the other fields investigated. The important question, as far as the future of Indian science is concerned, is whether the drop in output is due to an overall decrease in the number of papers published or whether it is due to a widening gap between the standard of science as practised in India and the standard of science elsewhere.

NEW WORLD

Nixon Unveils His Energy Policy

by our Washington Correspondent

PRESIDENT Nixon did what was expected of him last week by lifting restrictions on import of oil into the United States, proposing an end to regulations of the price of natural gas and urging swift exploitation of oil and gas deposits which lie under the outer continental shelf. The actions and proposals were outlined in his long overdue message on energy policy, which was finally unveiled last week and which shared the front pages with the Watergate affair.

President Nixon's proposals seem to have pleased most people, but they have been criticized by some for failing to take just account of environmental concerns, and by many for failing to provide more money for energy research and development.

The energy message is the most comprehensive declaration so far of the Administration's plans to deal with what has become popularly known as the great energy crisis in the United States, but which President Nixon prefers to call an "energy challenge". The basis of the energy challenge, problem or crisis is by now well known. Stated simply by Mr Nixon last week, it boils down to the fact that US "energy demands have grown so rapidly that they now outstrip our (US) available supplies, and at the present rate of growth, (US) energy needs a dozen years from now will be nearly double what they were in 1970". With 6 per cent of the world's population, the United States consumes about a third of the energy used in the world.

The upshot of the difference between domestic supply and demand is that the United States has found itself importing large amounts of fossil fuels, chiefly oil from the Arab states. In 1967, the United States ceased to be self-sufficient in oil, by 1972 it was already importing nearly 30 per cent of its total supply, and by 1985, it is estimated that import of oil will account for between 50 and 60 per cent of supply, 30 or 40 per cent of which will come from the Middle East. According to an estimate given last November by Peter G. Peterson, then Secretary of Commerce, by 1980 the US trade deficit in energy will amount to between \$15,000 and \$21,000 million a year.

In view of such figures, it is small wonder that many people have been crying "crisis". But energy shortages really hit home last Winter when supplies of fuel oil dried up in the Midwestern

states, and for many others the energy crisis will become a reality later this year when some filling stations run out of gasoline. Such short term disruptions of supply are, however, a slightly different problem from the overall energy supply and demand situation.

Briefly, what happened last Winter stems partly from a shortage of natural gas, which started a couple of years ago. As deliveries began to dry up, many electricity suppliers switched to oil, precipitating a shortage there and refineries churned out fuel oil as fast as they could and produced less gasoline. The effects of that will be seen later this year. And then there have also been problems of siting refineries and lack of capital for investment in the oil industry.

At present, the energy picture is dominated in the United States by the three fossil fuels—oil, natural gas and coal—and each has its own special problems. Oil now supplies about 46 per cent of energy consumed in the United

States, but demand is hopelessly outstripping domestic supply. Natural gas has about 32 per cent of the energy market, but its wellhead price has been pegged at an unrealistically low rate by the federal government and this has caused demand to soar while at the same time it has not been profitable for gas companies to prospect for new supplies and proven reserves have dwindled. The result: shortages have developed and pipeline companies have curtailed some supplies. Finally, the United States has about half the world's coal reserves, but persistent environmental problems associated with strip mining and with air pollution have been partly responsible for the fact that coal's share of the energy market has dropped from more than half immediately after the war to about 17 per cent today.

This is what President Nixon last week proposed to do about the supply of the three chief fossil fuels. As far as oil is concerned, he proposed to increase

NSF

Congress's Priorities

by our Washington Correspondent

CONGRESS has consistently voted in the past few years to give the National Science Foundation large increases in its budget for education and graduate student support programmes. But the White House has been equally consistent in refusing to spend the extra money. That particular pantomime will not occur again if the House subcommittee on Science, Research and Development gets its way, however, for the subcommittee last week passed a bill which, among other things, would severely restrict President Nixon's power to impound funds voted for the NSF.

The measure, which is tucked away in the annual authorizations bill for the National Science Foundation, would allow President Nixon to withhold funds from the NSF only if each of the foundation's programmes is reduced by the same proportion. In other words, if the measure survives the rest of the Congressional mill intact, the White House would no longer be able to withhold money from specific NSF programmes.

The full committee on Science and Astronautics will consider the subcommittee's proposal early in May, after the Easter recess, and is expected

to pass it. But the prospects in the Senate are less certain. For one thing, the authorizations bill will be dealt with by Senator Kennedy's NSF subcommittee, and Kennedy may not be willing to agree with a measure which would still allow President Nixon to impound money without first having to get Congressional approval.

In the continuing battle between Congress and the White House over who has final control over the Federal government's pursestrings, the Senate has agreed on an approach, spearheaded by Senator Sam Ervin, which would force Nixon to get Congressional approval each time he impounds any money. Kennedy would thus probably want a stronger anti-impoundment measure than one which tacitly assumes that the President has the right to impound money, even though it places considerable restrictions on how the money could be impounded.

Kennedy's subcommittee will probably hold hearings on the NSF budget early in May. During hearings before John Davis's Science, Research and Development subcommittee earlier this year, Dr H. Guyford Stever, director of the NSF, took exception to the anti-impoundment proposal because its inflexibility would prevent the foundation from altering its spending patterns as conditions change. But spokesmen from education associations welcomed the move.

short-term supply by scrapping a system of import quotas established in the 1950s. The result should be to increase imports of oil, particularly from the Middle East. For the slightly longer term, he urged Congress to clear away the legal barriers preventing construction of the Alaska pipeline and directed the Secretary of the Interior to step up the leasing of the Outer Continental Shelf for oil exploration and to allow leasing beyond the 200 metre isobath. It is estimated that the outer shelf holds about 186,000 million barrels of oil (about 45 years supply at the present rate of consumption) which is recoverable with existing technology. As for Natural Gas, President Nixon has proposed that Congress should amend the Natural Gas Act to deregulate the well-head price of gas so that the price will increase and make exploration more profitable. Finally, he urged Congress to pass the Administration's bill to regulate strip mining so that uncertainties will be removed and coal companies can get on with stripping coal from the land, and hopefully clear up afterwards. He also suggested that states should ease their rules for meeting secondary air quality standards specified by the Clean Air Act, to enable coal with a relatively high sulphur content to be burned (the secondary standards are designed to preserve general welfare by preventing damage to plants and materials) and pointed out that his budget increases the amount of money devoted to removing sulphur dioxide from stack gases and sulphur from coal.

As far as nuclear energy is concerned, President Nixon announced no new policies, but reiterated the Administration's plans to give the liquid metal fast breeder reactor high priority in the energy supply picture. Nuclear energy at present provides only about 1 per cent of energy consumed in the United States, but is expected to increase its share to about 30 per cent by the year 2000.

So much for the supply side of the equation for the next few years, but what about the demand for energy? Energy consumption in the United States has been soaring since the war, but only recently has there been serious discussion of curbing demand. President Nixon's message last week had many pious words to say about the need for energy conservation, but his two chief proposals—that an Office of Energy Conservation should be established in the Department of Interior and that manufacturers of energy consuming appliances should voluntarily label their products with respect to their energy use—are unlikely to make a big dent in demand. A more direct but less politically appealing means of reducing demand would be to increase energy prices, but the proposals announced last

week, with the exception of deregulating natural gas, are more designed to ensure adequate supplies of cheap energy.

The loudest blast of criticism directed at the energy message has focused on the fact that it provides no new money for research and development for new sources of energy. Instead, President Nixon merely reiterated that his budget for the 1974 fiscal year increases expenditures on energy research to \$772 million, up from \$642 million this year. That amount is clearly unsatisfactory to many members of Congress, however, for Senator Henry Jackson recently introduced a bill cosponsored by nearly thirty colleagues, calling for expenditures of \$2,000 million a year over ten years on non-nuclear research and development, and a task force in the House of Representatives has also called for an extra \$1,000 million a year. In reply to his critics, however, Mr Nixon said "it is foolish and self-defeating to allocate funds more rapidly than they can effectively be spent".

Other criticisms have come from environmentalists, annoyed at Mr Nixon's suggestion that the states should go easy on implementing the Clean Air Act, and critical of plans to exploit offshore oil and gas reserves. Offshore oil wells have had a bad name ever since a blowout in a rig in the Santa Barbara Channel dumped a huge oil slick on the California coastline a few years ago. But Mr Nixon said in his message that "new techniques, new regulations and standards and new surveillance capabilities enable us to reduce and control environmental dangers substantially". He also announced that he has asked the National Academy of Sciences to study the environmental effects of possible oil production in the Atlantic Outer Continental Shelf and in the Gulf of Alaska. Environmentalist groups are also disappointed that the energy message did not increase funds for development of clean sources of energy such as solar power.

Finally, as far as organization of energy policy and programmes in the Federal government is concerned, the energy message announced that President Nixon will soon send to Congress a plan for reorganizing executive departments along functional lines. This will include the setting up of a Department of Energy and Natural Resources, which will take in energy programmes of existing agencies, including the civil power programmes of the Atomic Energy Commission. The focus for the new Department's responsibilities for energy will be in the present Department of Interior, for Mr Nixon said last week that he has directed the department to strengthen its organization of energy activities and its "capabilities for overseeing and coordinating a broader range of energy research and development".

A move to strip the civilian power programme from the AEC would, of course, turn the agency into little more than a weapons supplier for the Defense Department.

FOETAL RESEARCH

An Abrupt Ban

by our Washington Correspondent

The difficult ethical question of whether the Federal government should sanction research on live, aborted fetuses which have no chance of survival for longer than a few minutes, was abruptly settled last week. Dr Robert W. Berliner, Deputy Director of the National Institutes of Health for Science, told a crowd of Catholic high school students that he knows "of no circumstances at present or in the future, which would justify NIH support of research on live, aborted fetuses". The students had descended on the NIH campus in protest two days after the Washington Post disclosed that the ethics of such research are under discussion at NIH.

Although Berliner's statement was a personal one, he made clear that it was supported by other NIH officials, including Dr John Sherwood, Acting Director of NIH. And NIH spokesmen have since confirmed that no research will be carried out on live, aborted fetuses with NIH funds, at least until a committee which is examining a range of ethical issues involved in human experimentation has had a chance to report on the matter.

The committee, which has been meeting under the chairmanship of Dr Ronald Lamont-Havers, Deputy Director of the National Institute of Arthritis, Metabolism and Digestive Diseases, should produce its recommendations in two or three months' time. But Berliner's statement and the attitudes of top NIH officials seem to have prejudged official reaction to whatever the committee recommends.

Berliner also said at his meeting with the Catholic students that the NIH is at present funding no research on live fetuses and in fact little such research has been possible in the United States for two chief reasons. First, mid-term abortions are usually carried out in the United States by saline injection which damages the foetus so that it is dead when aborted. Intact foetuses, whose hearts are beating but which are too immature to live for more than a few minutes unless kept alive artificially, are usually the result of delivery by caesarean section. The second obstacle to such research was that until the Supreme Court recently removed all legal barriers to abortion within the first three months of pregnancy and allowed considerable discretion about mid-term abortions, many states outlawed abortion completely.

NEWS AND VIEWS

The Importance of Being Plume Conscious

It is not difficult to identify thermal plumes in the mantle as the latest geophysical concept to be taken up in a big way; and yet like any important, albeit essentially simple, idea whose time has come, it is not entirely new and without history. Rather more than the germ of the idea can be traced back at least a decade to Tuzo Wilson's (*Nature*, 197, 536; 1963; and *Canad. J. Phys.*, 41, 863; 1963) pioneering work on the space-time relationships of oceanic islands. In those days, of course, the seafloor spreading hypothesis itself was new, not yet having received support from the analysis of oceanic magnetic anomalies, and even continental drift was far from the near-universal acceptance it has today. Convincing evidence in favour of these phenomena was still being sought in the knowledge that exploration of the ocean floors was likely to play an important part; but with access to the ocean floors limited, only oceanic islands offered usable data.

Tuzo Wilson's starting point was the realization that most truly oceanic islands were less than 100 million years old as represented by the ages of the oldest rocks then found upon them—a discovery which implied that such islands were either atypical of ocean floor regarded as old by geological tradition or representative of ocean floor necessarily young as a consequence of continental drift. The choice between these alternatives has long since been made on the basis of a wide variety of evidence including that from the oceanic islands themselves. What Tuzo Wilson did was to show that the distance of islands from the nearest oceanic ridge in the Atlantic and Indian Oceans increases generally with the age of the islands; and this he took to be consistent with the view that ocean floor steadily moves away from the ridges. The explicit assumption here (supported by the presence of active or recently active volcanic islands such as Jan Mayen, Iceland, the Azores, Ascension, Tristan da Cunha and Bouvet along the Atlantic ridges) was that oceanic islands generally originate over the oceanic ridges but lose their activity as they move away from the source of magma represented by upwelling convection currents beneath the ridge axes.

But whereas this assumption seemed to be justified generally in the cases of the Atlantic and Indian Oceans, the oceanic islands of the Pacific told a different, or at least modified, story. Thus the simple age-distance pattern was not apparent in the Pacific; and although it was possible to conceive of reasons for the absence of such a pattern which were still consistent with a ridge origin for oceanic islands, Tuzo Wilson chose instead to examine in some detail an alternative origin for Pacific islands, and thus suggest an important source of magmatic activity unrelated directly to either oceanic ridges or submarine faults.

The key to the problem was the way in which all active and recent volcanoes of the Pacific basin lying far from land and beyond island arcs apparently form linear chains trending west north-west to the west of the East Pacific Rise and east to the east of the Rise. Up to the early 1960s, these chains of islands and seamounts were widely

interpreted as the results of lava extrusion from large faults in a rigid Earth. Although a progression of ages along each chain was recognized, the total age spread was not thought to be great and so it was just conceivable that the volcanic activity spread slowly as each fault gradually extended. Unfortunately, not only did the nature of the faulting prove impossible to determine but the range of ages along each chain turned out to be greater than previously supposed. Thus although each volcano in a chain had a similar history, the end members had reached quite different stages in that history separated by tens of millions of years.

Tuzo Wilson's solution to this dilemma was to suppose that the Pacific chains with their progression of ages were, as the Atlantic and Indian Ocean islands with their own age-distance pattern, manifestations of a spreading ocean floor, but with the difference that the source of activity was no longer necessarily an ocean ridge. Thus in terms of the convection current hypothesis, the magma source was seen as the relatively slow-moving interior of a convection cell whose surface was moving along above at a faster rate. Once a volcano formed on the ocean floor it would be gradually moved away from its initial position by the faster-moving part of the convection cell and become extinct, leaving virgin territory for a new, younger volcano to develop. The source of each volcano remains the same, and so each volcano has a similar history shifted only in time. Moreover, it is not necessary for the source to be immobile; it need only move more slowly than the current above.

It is clear that Tuzo Wilson was, in effect, proposing the existence of what would now be called hotspots to explain islands of the Hawaiian type—surface manifestations of non-linear magmatic sources unrelated directly to the apparently more important linear sources represented by oceanic ridges. Moreover, there is a peculiar irony involved in that, in a particular sense, Tuzo Wilson's concept is closer to present day ideas than it might have been had it been proposed much more recently. To many, and perhaps most, geophysicists, the direct translation of Tuzo Wilson's proposals into present day terms would probably involve placing the magma source in the asthenosphere. This poses conceptual problems in that, on this basis, the source would be moving faster in the asthenospheric convection currents than the resulting volcanoes at the surface of the overlying lithosphere. Tuzo Wilson, however, was writing at a time when convection currents were regarded, if at all, as mantle wide. Thus his source could be placed deeper in the mantle, if necessary, and so, in retrospect, accord more closely with the origin of today's thermal plumes.

In the event, of course, the hotspot idea has been developed, most notably by Morgan (*Nature*, 230, 42; 1971), in terms of magma sources largely independent of the asthenosphere in the direct sense, and in so doing has acquired several important characteristics apparently not

previously envisaged. For one thing, a worldwide system of hotspots is now envisaged as distinct from ridge activity, unifying conceptually sources of volcanic islands (and even continental volcanic centres) and aseismic ridges. Thus Iceland, situated on an oceanic ridge, is thought to be the result of a hotspot no less than the Hawaiian chain, which lies far from a ridge. Second, Morgan has proposed that hotspots are the surface manifestations of deep mantle plumes which bring up heat and "relatively primordial" material to replenish the asthenosphere, although it is not entirely clear yet whether this can be regarded as the sole source of the viscous asthenospheric material ultimately returned to the deeper mantle. But Morgan's most far-reaching suggestion is that plumes/hotspots provide the principal driving force for plate motions. Upwelling material along oceanic ridges and downwelling material along trenches, by contrast, represent near-passive processes contingent on the effects of the plumes. In other words, the primary manifestation of sub-lithospheric convection is the localized plume-hotspot rather than the large-scale cell mirroring the rise-trench system.

These are radical suggestions indeed; and although it is more than two years since Morgan made them, it is doubtful whether many geophysicists have yet fully appreciated just how fundamental is the implied impact on concepts relating to the Earth's global behaviour. To be sure, there has recently been a flurry of activity in which thermal plumes and hotspots have been invoked to prove or disprove the existence of unrelated phenomena such as polar wandering; but such studies have been hardly more than extensions in the use of Tuzo Wilson's original chain concept with the added assumption that hotspots are fixed relative to the mantle. The problem in assessing the validity of hotspots is, of course, that so little direct information about them is available. On page 565 of this issue of *Nature*, however, Schilling reports the results of a detailed geochemical investigation into the spatial and temporal characteristics of the supposed Iceland plume. In view of the potential significance of the plume concept, Schilling's important article deserves to be studied by all Earth scientists whatever their particular specialisms might be.

P. J. S.

Microarchaeology

ARCHAEOLOGICAL excavators are now experiencing advances in technique more radical than any since the 1920s and 1930s. Traditionally the role of the excavator has been to seek significant artefacts and the buried traces of former structures with which they are associated, enclosed and contained within the debris and earth of the archaeological site. Today, however, the recovery of such artefacts has become rather routine, and it is the information afforded by this debris and by the earth itself which is becoming a new focus of interest. A battery of techniques now being developed makes the processing of this material quite as informative as the study of the artefacts themselves. Equipment for flotation and sieving is taking its place beside the trowel and the spade in the indispensable armoury of the digger.

It is now some fifty years since Sir Mortimer Wheeler established definitively the principles of controlled stratigraphic excavation, admirably set out in his *Archaeology*

Works of Man

MORE than half a century ago, in 1921, the Swedish geologist Dr J. G. Andersson, noticed small fragments of white vein quartz among material that had fallen from an exposure of a fissure deposit into a quarry at Choukoutien in the region of Peking. The significance of the occurrence was immediately obvious to him: quartz was "exotic" at this locality. He turned to his companions and explained dramatically: "Here is primitive man, now all we have to do is to find him." (One of Andersson's companions on this occasion was Dr O. Zdansky, who discovered the first fossil human teeth at this site, which later became famous as the type locality of Peking Man, *Sinanthropus pekinensis*, now known as *Homo erectus pekinensis*, of Middle Pleistocene antiquity.)

It is not without interest that the milky white vein quartz which was worked by primitive man in the Omo Valley, Ethiopia, about 1.5 million years earlier in time than the material at Choukoutien (which is between 400,000 and 200,000 years old) is also "exotic". At Omo, as at East Rudolf in Kenya (the site of an even older culture, see Isaac, *Science*, **173**, 1129; 1971), man by good fortune has been found before or concurrently with his tools. Remains of hominids were recovered from the Omo Valley in the late 1960s, and now Merrick, and his colleagues, De Heinzelin, Haesaerts and Howell, record, on page 572 of this issue of *Nature*, their discoveries in 1971 and 1972 of artefacts *in situ* in the upper members of the Shungura Formation. Only the activity of man, 2.0 million years ago, as Merrick *et al.* point out, can feasibly account for the localized concentrations of the quartz tools in the fine clay sediments. Indeed the quartz would have had to be transported from sources several kilometres distant.

These artefacts from Omo are about 0.6 million years younger than the pebble tools and flakes uncovered in 1969 and 1970 *in situ* at East Rudolf. These two primitive cultures, and that found in Bed I, Olduvai Gorge (known as Oldowan), together with their associated hominids and fauna, now provide opportunities for a comparative study of early palaeolithic man and his habits in East Africa during a span of several hundreds of thousands of years.

From a Correspondent

from the Earth (Clarendon, Oxford, 1954) and more than thirty years since Bersu demonstrated in Britain the techniques of area excavation (*Proc. Prehist. Soc.*, **7**, 30; 1940), where large expanses of the site are cleared, without intervening baulks, to reveal the entirety of whatever structures may be indicated by postholes or other scanty traces. In such excavations, and indeed since the work of Schliemann a century ago, it was normal to collect the animal bones unearthed, or any obvious accumulations of carbonized grain, and submit them for specialist study. Such materials did not, however, become a principal preoccupation until twenty years ago, when the objective of much

research became the understanding of the origins of farming. But even then special recovery methods were not normally used for such materials, nor was very serious thought given to the effect of such techniques as were used on the quality or nature of the resultant data.

Perhaps the first systematic attempt to develop, on a deeply stratified site, recovery methods appropriate to such materials came in 1963 in the excavations at the early farming site of Ali Kosh in south-western Iran. All the excavated material was screened through a sieve, and selected samples were subjected to a water flotation process to recover carbonized grain. The flotation method for archaeological materials was first reported in the United States (Struever, *Amer. Antiq.*, **33**, 353; 1965), and modified at Ali Kosh by Hans Helbaek. As the excavators of Ali Kosh aptly commented: "While most Near Eastern archaeologists have dug for buildings and incidentally recovered a few seeds in the process, we dug for seeds and incidentally recovered a few fragments of buildings in the process" (Hole, Flannery and Neely, *Prehistory and Human Ecology of the Deh Luran Plain*, University of Michigan, Ann Arbor, 1969, p. 4).

Systematic sieving of a column sample is likewise an American innovation: it has for some time been used routinely on the shell middens of the Californian Indians, which are little more than mounds of discarded food refuse (Meighan *et al.*, *Amer. Antiq.*, **24**, 1; 1958), but only recently has it been applied coherently and systematically to a wider range of sites. In particular, water sieving has proved a particularly rapid and efficient means of recovering material, and a number of "machines" which systematize and speed the processes of flotation and sieving of the soil have come into use in the past two or three years (French, *Anatol. Stud.*, **21**, 59; 1971). They have been used to good effect by Dr David French, Director of the British Institute of Archaeology at Ankara in the institute's rescue excavations in the Keban area of east-central Turkey, and on his important and very early neolithic site, Can Hasan III.

It is now becoming clear, however, that the real significance of these uses is not simply in the increased recovery of material, but principally in the increased reliability of the data so

obtained as a basis for the quantitative reconstruction of early diet or, in the case of small artefacts, for a quantitatively valid assessment of the cultural equipment at the site. Several contributions to *Papers in Economic Prehistory* (edited by Higgs, University Press, Cambridge, 1972) make this point with great force. Two in particular, by Payne, demonstrate the great vulnerability of archaeological data to sample bias. Already the circumstances of burial and of preservation cause serious distortion: Payne has now shown clearly the severe bias that is introduced by considering material recovered by hand during excavation — animal bones for instance. Systematic recovery by sieving radically alters not only the quantities recovered, but the proportions of one species to another, whose estimation is, of course, the object of the exercise. Dennell, in the same volume, in discussing the interpretation of plant remains, shows how the results obtained depend greatly on the type of deposit recovered, whether from floors or middens, and emphasizes that only carefully controlled recovery procedures will permit of any distinction between these. Few archaeologists in the future are likely to accept as reliable any assertion

about early diet that is not based, at least in part, on the results of controlled sievings, themselves carefully interpreted.

Column sampling, for long regarded as an inadequate means of archaeological recovery, is in consequence of such studies again being found useful. It is, of course, a well established procedure in pollen analysis, and has previously been used for this purpose on archaeological sites lying in peat (Clark and Godwin, *Antiquity*, **36**, 10; 1962). And archaeologists are now coming to realize that an understanding of the processes by which the site was formed, geomorphologically, can make a significant contribution. This is now seen to apply, not simply to the great depths of natural deposit which often cover palaeolithic sites and which have for long been studied by Quaternary geologists, but to the cultural levels themselves which were formed as the result of natural as well as human activities. The tell mounds of the Near East, for instance, have never been investigated geologically or geomorphologically, although they are very prominent features of the landscape. The problem of their formation has now been tackled by Davidson (*Archaeometry*, **15**, 143; 1973) in an article which will surely

Function of Nuclear RNP Particles

THE presence of ribohomopolymer regions in nuclear heterogenous high molecular weight RNA (HnRNA) and in cytoplasmic mRNA has been recognized for some time. Although it is known that the synthesis of ribohomopolymers is a post-transcriptional event, the precise timing and location of the process are uncertain. The function of these homopolymer regions is also unknown, although it has been suggested that they have a role in the processing of HnRNA to produce cytoplasmic mRNA. It is satisfying, then, to read the report of Niessing and Sekeris in next Wednesday's *Nature New Biology* (May 2) which describes two enzymatic activities and a ribohomopolymer synthetase activity in rat liver nuclear ribonucleoprotein (RNP) particles.

Niessing and Sekeris investigated these enzymatic activities *in vitro* using isolated 30S nuclear RNP particles. Optimal incorporation of ATP, CTP, GTP and UTP into homopolymers was achieved at 15 mM Mn^{2+} . ATP incorporation into poly (A) was also obtained at 5 mM Mn^{2+} and at 12 mM Mg^{2+} . These three enzymatic activities have

distinct pH optima. Examination of the reaction products by high voltage electrophoresis of alkaline hydrolysates indicates that the ribohomopolymers are not synthesized *de novo* but are formed by the successive addition of nucleotides to a pre-existing polynucleotide. Homopolymer formation is independent of DNA but addition of rat liver nuclear RNA stimulates the reaction three-fold. Treatment of RNP particles with RNase eliminates ribohomopolymer formation. Thus it is probable that the HnRNA contained in the RNP particles serves as a primer for synthesis of ribohomopolymers. Inhibitors of transcription have no effect on the enzymatic activities of the homopolymer.

Niessing and Sekeris have previously found in the protein moiety of RNP particles an enzyme which cleaves HnRNA. The discovery now in RNP particles of ribohomopolymer synthesizing enzymes which operate at a post-transcriptional stage independently of DNA points strongly to the role of RNP particles as the sites of cleavage and further processing of HnRNA to produce mRNA in its cytoplasmic form.

be the first of many geomorphological studies of archaeological sites. By systematic sampling of the 11-metre stratigraphic column at the neolithic settlement mound at Sitagroi in north Greece, a sequential series covering the 3,000 year duration of the site was obtained. Phosphate and particle size analyses were undertaken and these show that the build-up of material on the site resulted from the deposit of material transported there solely as the result of human activity. They also suggest that the occupation of the site was interrupted for long periods, which has a significant bearing on the archaeological interpretation. Indeed such investigations give a foretaste of the day, to which one must now look forward, when each of the different layers which the archaeologist records on stratigraphic profiles and sections will be interpreted for him in detail by the geomorphologist.

These different developments, with their emphasis on controlled techniques of recovery, herald, it would seem, a new phase of cooperation between the excavator and the specialists. It is no longer enough for the archaeologist to take a few samples and send them back to the laboratory for analysis: this may suffice for some purposes, such as radiocarbon determinations, but not in a coherent study of the site as a whole. Other techniques for the study of the environment are currently being developed (cf. Evans, *Land Snails in Archaeology*, Seminar Press, New York and London, 1973) and these, and investigations of diet, can only be effected by specialists on the spot. Archaeological excavation, at least at the micro-level, in the recovery of samples for systematic quantitative interpretation, has become too complex a business to leave to the archaeologist alone.

From our
Archaeology Correspondent

PLANT CELL WALLS

New Structural Model

from our Phytochemistry Correspondent

PLANT cells are encased in a rigid wall in which cellulose, hemicellulose, pectic polysaccharide, protein and lignin are the principal structural components. These walls are of fundamental importance because they are necessarily intimately involved in all aspects of the growth, development and morphogenesis

of plant cells. Cell growth is not possible without wall growth and thus an understanding of cell wall structure is a vital prerequisite to a deeper understanding of the regulation of development. A significant contribution in this direction is to be found in the latest issue of *Plant Physiology* in the form of a series of three long but cogently argued articles by Albersheim of the University of Colorado and his colleagues Talmadge, Keegstra and Bauer (51, 158; 174 and 188; 1973). In this impressive *tour de force* the structure of the primary wall of suspension-cultured sycamore cells is described and a new and highly intriguing model is proposed. The work was made possible by the availability of purified hydrolytic enzymes and by improvements in the techniques of methylation analysis using combined gas chromatography-mass spectrometry.

The results show that the primary walls of sycamore are composed of 10 per cent arabinan, 2 per cent 3,6-linked arabinogalactan, 23 per cent cellulose, 9 per cent oligo-arabinosides attached to hydroxyproline, 8 per cent 4-linked galactan, 10 per cent hydroxyproline-rich protein, 16 per cent rhamnogalacturonan and 21 per cent xyloglucan. The structures of the pectic polysaccharides and the hemicelluloses were determined by methylation analysis and form the basis of the first two articles and the third report deals with enzymological experiments carried out to determine the nature of the linkages between the various macromolecular constituents. By the sequential use of endopolygalacturonase, endoglucanase and protease, evidence has been derived for covalent linkages between the pectic polysaccharides and both the xyloglucan

and the structural protein. Purified xyloglucan binds strongly to isolated cellulose, but by hydrogen bonding, not by covalent connexions.

Based on these observations a new model of the structure of the sycamore primary cell wall is proposed in which the cellulose elementary fibrils are connected to the matrix by hydrogen bonds between cellulose and xyloglucan. The latter, in its turn, is crosslinked by covalent bonds to the other constituents through the pectic polysaccharides which are themselves attached to the structural protein, probably by means of 3,6-linked arabinogalactan chains attached to the seryl residues of the protein. Furthermore, the hydroxyproline residues of the structural protein are thought to be glycosylated by arabinosyl tetrasaccharides.

The attraction of this new model is that it immediately lends itself to conjecture with regard to one of the chief unsolved problems of plant physiology—the mechanism of auxin action. It is generally agreed that growth stimulation by auxin involves the rapid weakening of the primary cell walls. This process must occur by the sliding of cellulose elementary fibrils past each other. This weakening and sliding may involve enzymatic breakage of the crosslinks in the walls, but, as the authors point out, the new model offers a more attractive alternative.

The only non-covalent link between the structural polymers of the wall is the hydrogen-bonded connexion between the xyloglucans and the cellulose, and if the xyloglucans and the cellulose could move relative to one another extension of the wall would result. The authors propose an ingenious non-enzymatic creep in which the xyloglucan

Symmetric Transcription of SV40 DNA

LAST year Aloni reported that during the replication of simian virus 40 (SV40) in permissive monkey cells at least part of the viral duplex DNA is transcribed symmetrically to yield RNA molecules that are complementary to each other. For two reasons this finding was most unexpected. First, in most other systems that have been investigated in detail transcription is apparently asymmetric and involves only one strand of duplex DNA. Second, the SV40 RNA molecules that can be isolated from cytoplasmic polysomes of infected cells are not self complementary and at first sight seem to be the products of asymmetric transcription. To reconcile symmetric transcription with the accumulation in the cytoplasm of asymmetric RNAs that are not self complementary, one must postulate that the complementary RNA chains produced by symmetric transcription are partially

and specifically degraded. In *Nature New Biology* next Wednesday (May 2) Aloni reports further experiments probing these events.

He has confirmed his original finding that late in the lytic cycle of SV40 in monkey cells newly made SV40 RNA is self complementary. He has also shown that this RNA contains poly (A) tracts which are almost certainly added after transcription, not least because SV40 DNA does not contain poly (dA) or poly (dT) tracts. Combining his own data with those of others, Aloni proposes that at least 60 per cent of the SV40 genome is transcribed symmetrically, that poly (A) is then added to the 3' end of the complementary RNA molecules, and finally that each RNA molecule is partially degraded from its 5' end to yield non-complementary viral messenger RNAs which have 3' poly (A) tracts.

moves "like an inchworm" along the cellulose fibre, requiring only the simultaneous breaking of about four adjacent hydrogen bonds. This concept is capable of explaining the action of auxin on the basis that auxin activates a hydrogen ion pump within the cell membrane increasing the H^+ content of the wall; the lowering of the pH should enhance the loosening of the hydrogen bonds and thus make the postulated creep process more active.

PROTEINS

Hunting the Helix

from our Molecular Biology Correspondent
W. H. AUDEN has described a professor as one who talks in other people's sleep, and such also seems by now to be the fate of that intrepid body of theoreticians still tirelessly labouring at the deduction of protein folding from amino acid sequences. The effusions in this field are abundant, usually very hard to understand, and at least on casual inspection pose little threat to the crystallographers. From time to time, however, most protein chemists will probably feel an obligation to grit their teeth and try to find out for themselves how matters stand. In regard to globular protein conformation, the proposition that all generalizations are untrue can fairly be said to hold, and thus far the best that has been achieved is some quantitative expression of the observations that met the eye when the first globular protein structure was determined. A number of recipes for the identification of the sequences that will assume the α -helical conformation have been devised, and the most searching statistical analysis—that of Robson and Pain—has had a very considerable degree of success.

Nagano (*J. Mol. Biol.*, **75**, 401; 1973) now descants on this theme with an analysis for the three most prevalent conformations, α -helices, β -structures, and the loop or β -bend. He has searched for correlations with combinations of different side chains taken pairwise at separations from zero up to six residues along the chain. Whereas simple rules of thumb, in particular Edmundson's helical wheel, predicate that it is the third or fourth residue along the chain that determines whether or not the α -helix will be favoured, Nagano finds that the most important correlation implicates the next nearest neighbour. For loops, on the other hand, long range interactions become more important. The predictions of α -helicity and of pleated sheet structure are nearly 90 per cent correct on a favourable definition of error, and that of loops, which is harder, 64 per cent on the same basis.

Wu and Kabat (*ibid.*, 13) advocate a different formula for predicting the positions of α -helices in a sequence; instead of taking the residues singly, or pairwise, or in a combination of both, they consider instead triplet sequences along the chain. They have collected data on the backbone dihedral angles, which define the local conformation, from a series of proteins of known sequence and structure, in which are to be found 1,561 of the 8,000 possible triplet sequences. The favoured combinations of the two variable dihedral angles fall into a few zones in the conformational map, and for a sequence of a protein of unknown structure it is, therefore, probably adequate to assign values to the pair of dihedral angles for each peptide bond within a margin of 15° , in accord with values for both of the dipeptide and the three tripeptide sequences of which it is part. Using horse heart cytochrome *c* as test sequence, Wu and Kabat report that they can adequately match the observed dihedral angles in this protein (which is very low in α -helix) for 56 of the 103 peptide bonds. The answer is wrong for 17, and somewhere in between for 29. In some of the former, the information on the relevant triplets is scanty,

and others were in the vicinity of the haem group, and therefore arguably perturbed by this intruder. There is a marked tendency towards overselection of values in the α -helix range, which may to some extent be rationalized, for instance, by the unusually high helicity of some of the proteins used in compiling the reference data. Wu and Kabat suggest that where one is concerned with a family of homologous proteins, such as especially the immunoglobulins, the existence of different sequences with presumably the same dihedral angles allows a much better prediction of local conformation, which will serve until, as they say, high-resolution X-ray data became available.

Meanwhile, Ponnuswamy, Warne and Scheraga (*Proc. US Nat. Acad. Sci.*, **70**, 830; 1973) have continued the attempt to predict chain conformation by working outwards from each residue to take into account interactions with progressively more remote side chains. With parts of lysozyme as models, they find that the agreement with the real structure improves as the length of the peptide encompassed in the calculation increases. That is to say, progressively more areas of the conformational map are excluded, and the dihedral angles

Antigen Specific Product of T Cells

T LYMPHOCYTES, those of thymic origin, are versatile in that they seem to be capable of both helping B lymphocytes (of bone marrow or bursal equivalent origin) to synthesize antibody and being cytotoxic effector cells. Many of the analyses of the mechanism of homograft or tumour immunity postulate a central role for T cells, but there arise problems from their postulated duality of action. It is known from *in vitro* experiments that T-cell cytotoxicity can be blocked by various factors, for example, antigen, antigen-antibody complexes and antibody. The first two of these are probably capable of blocking lymphocytes by binding to their antigen receptors; the last two can probably efface the target by binding to its antigens.

The production of antibody and of the antibody component of complexes may require a T-cell helper effect. Thus T cells can both kill cells and facilitate the production of some of the factors which can prevent T-cell killing. In next Wednesday's *Nature New Biology* (May 2) Röllinghoff *et al.* indicate that either during cooperation or the response to a malignant isograft, T cells synthesize a specific protein which may help eventually to devise a common mode of action for T cells in what at present seem to be two disparate functional attributes.

Röllinghoff *et al.* took thymocytes

from Balb/c mice and incubated them with Balb/c irradiated plasma-cell tumour cells for seven days. Dead cells were removed and the remaining cells (T cells as adjudged by their sensitivity to an anti-theta antiserum) were subjected to a procedure whereby their surface proteins were radioiodinated. Subsequently proteins released from the cell surface were collected in the supernatant and their binding capacity for the specific plasma-cell tumour was determined. It was found that an IgM type protein released from the activated T-cell surface had 5–20 times greater binding capacity for the plasma cell tumour against which it had been stimulated than for a comparable but different syngeneic plasma-cell tumour. No binding cell surface immunoglobulin was obtained from non-stimulated T cells.

Other workers have shown that 7S IgM molecules are produced by co-operating T cells, and the present finding, if it can be substantiated, seems to indicate that production of an antibody by T cells is part of their mode of response to both transplantation and particulate or soluble antigens. The experiments do not indicate, however, whether the immunoglobulin produced by the activated T cells contributes to their cytotoxicity, nor is there evidence presented that any cytotoxicity is generated against the plasma cell tumour.

corresponding to minimum computed energy are not necessarily the same for a bond in a dipeptide as for the same bond in the nonapeptide, for example. Ponnuswamy *et al.* assert that at a chain length of nine the conformation begins to home in on the true geometry, so that long-range interactions between remote parts of the chain need often not be considered. The results, however, show that there is still a long way to go. The unpalatable fact remains that the experimental conformation of excised peptide segments is all but unrelated to their state in the native protein, so that failure to take account of long-range interactions is by no means a trivial limitation.

Crawford, Lipscomb and Schellman (*ibid.*, 538) have concentrated on the hairpin turn of the polypeptide chain as a structural landmark suitable for purposes of prediction. This turn is defined by four residues, and Crawford *et al.* convey the surprising information that no less than 33 per cent of the residues of the globular proteins in their sample are engaged in such formations, as against 34 per cent in helices and 17 per cent in pleated sheets. Asparagine, glycine and proline have a strong tendency to appear in the reverse turns. They are most often in the first, second and third position respectively, and tryptophan is frequently found in the fourth. A case for a special function for leucine residues is made by Chou and Fasman (*J. Mol. Biol.*, **74**, 263; 1973), who give experimental evidence that this residue confers the greatest stability on polypeptide α -helices. Its occurrence is most frequent in internal helices in globular proteins, and around the walls of clefts containing prosthetic groups, and Chou and Fasman suggest that it may have an important structure nucleation function.

On the nucleation of chain folding, Wetlaufer (*Proc. US Nat. Acad. Sci.*, **70**, 697; 1973) has found on examining the structures of many globular proteins that spatially separate compact folded regions are often present, by criteria which he defines, and he gives arguments why it might be anticipated that chain folding would ensue after the formation of independent conformational nuclei in each separate domain.

Some potential energy calculations on β -pleated sheets have been done by Chothia (*J. Mol. Biol.*, **75**, 295; 1973), who finds that a right-handed twist in the direction of the polypeptide chain, which corresponds to dihedral angles within one zone of the conformational map, makes for minimization of energy. This sense of twist turns out to prevail in β -structures in known globular proteins. A structure with a left-handed twist would have appreciably greater conformational freedom, with relatively large variability of dihedral angles.

PALAEOMAGNETISM

Dating Hadrian's Wall

from our Geomagnetism Correspondent

THE success of palaeomagnetism rests on the fortunate fact that many rocks contain magnetic grains which have very high coercive forces. When a rock forms, its magnetic components acquire a magnetization in the direction of the ambient geomagnetic field; and because many of the grains have high coercive forces, the magnetization becomes "locked in" to the rock, usually at temperatures between 500° and 700° C, and is thereafter largely unsusceptible to the vicissitudes of subsequent history. Accordingly, many rocks still contain a valid record of the direction of the geomagnetic field at the time of their formation, perhaps thousands of millions of years ago. The palaeomagnetist simply has to measure such directions to determine the properties of the Earth's magnetic field in the past.

Unfortunately, this task is frequently complicated by the fact that all grains do not have high coercive forces. The magnetic particles in any given rock sample are likely to cover a range of compositions, sizes and physical structures — all factors which determine coercive force — and thus a range of coercive forces. Grains throughout the whole of this range will acquire magnetizations in the direction of the original field; but grains with lower coercive forces will be less inclined to maintain the direction throughout time in the face of random temperature fluctuations. Thus if the orientation of the rock with respect to the external field changes (for

example, because of continental drift or because of field reverses), the grains with lower coercive forces may, and often do, lose their original magnetization and acquire a new one in the new field direction. In short, they are magnetically unstable and thus subject to viscous remagnetization which can take place in times ranging from seconds to hundreds of thousands of years.

To most palaeomagnetists, viscous magnetization is a nuisance to be removed (usually by thermal or alternating field demagnetization) before the more permanent record of the ancient magnetic field can be measured, although there have been numerous investigations of viscous magnetization as a phenomenon in its own right. Heller and Markert (*Geophys. J.*, **31**, 395; 1973) have, however, now attempted to put viscous magnetization to practical use in the dating of rock movements. The theoretical basis of their work is chiefly a series of investigations carried out by Néel (see, for example, *Phil. Mag. Suppl. Adv. Phys.*, **4**, 191; 1955) during the late 1940s and early 1950s. This theory is complicated, incomplete and probably defective in part, although with some modification and addition it has proved sufficient for the present purpose. Using the theory as a basis, Heller and Markert have developed a set of experimental procedures involving alternating field demagnetization to determine the time at which viscous magnetization began to form in any given rock.

The rocks chosen by Heller and Markert to test the method were quartz dolerites built into Hadrian's Wall near Greenhead in Northumberland, and they almost certainly originated from

Singularities and the C Field

ABOUT ten years ago Hoyle and Narlikar developed and published a model of the creation of matter which depends on a small and elegant modification of the field equations of general relativity. The modification was called the "C" field, a negative energy field whose gradient is not a conserved flux but has sources where matter is created. The motivation for this work was the steady-state cosmology according to which, in spite of the expansion of the Universe, the density of matter remains everywhere constant in time.

It is a measure of the rate of progress of observational cosmology that the original motivation for this work has largely disappeared; there are few cosmologists who now take seriously the model of the Universe which looks the same in every direction in every place at every time. But the need to modify the equations of general relativity remains, because the usual equations possess no solutions free from singulari-

ties. There is not even a model of a massive object collapsing under its own weight without an elastic repulsion. Penrose and Hawking and others have shown that singularities occur under quite wide and physically reasonable conditions. Several workers are seeking a quantization of the equations to avoid the singularity.

In next Monday's *Nature Physical Science* (April 30) Narlikar now points out that the classical modification made for the steady-state theory can also avoid the singularity. He presents the equations in a new way and works out two special cases. One is the steady-state theory with a constant matter density. The other is the big-bang model modified by the fact that the creation of matter at the origin is at the expense of the negative energy C field. The resulting geometrical structure is similar to that of a model with torsion put forward by Trautman (*Nature Physical Science*, **242**, 7; 1973).

the Upper Carboniferous to Lower Permian intrusions of the Great Whin Sill on the crest of which part of the wall was constructed. This is an ideal circumstance, for it is clear that all the dolerites were originally magnetized in the same direction because they came from the same body. Now, however, they are oriented randomly with respect to that direction and have presumably maintained their present positions since Roman times. Whatever viscous components the rocks may have had before removal by the Romans, the present viscous magnetizations must date from the construction of the wall. The Heller-Markert techniques thus effectively give an independent method of dating the building of Hadrian's Wall. An added bonus is that similar techniques may also be applied to the Great Whin Sill itself, the source of the dolerite in the wall.

According to Birley (*Hadrian's Wall*, Ministry of Public Building and Works, London, 1963), Hadrian's Wall was constructed during the first four centuries AD. According to Heller and Markert, the ages of the viscous magnetizations in three of the wall dolerites are 1,600, 1,800 and 2,400 years, respectively (average 1,933 years). The agreement is thus quite good, although the spread of measured ages indicates that the experimental techniques are subject to errors. It is perhaps unfortunate that the age of the viscous magnetization is apparently an exponential function of the parameters which are measured experimentally, so that any experimental error is magnified in the final result. For example, an experimental error of only 5 per cent will lead to error limits of 600 years and 3,600 years for the dolerite "dated" at 1,800 years. For this reason, and because of uncertainties in the basic theory, the agreement between palaeomagnetism and archaeology is really quite remarkable. But by the same token, if the Heller-Markert method is ever to be used widely, extremely high experimental accuracy will be required and statistical methods will have to be applied to large numbers of samples.

Finally, what of the Great Whin Sill itself? Because of the times involved in viscous remagnetization, it is evident that any viscous magnetization acquired in previous geomagnetic polarity epochs will have been completely erased by now. Thus as the Sill is older than the last major field reversal, its present viscous magnetization should date only from that reversal 700,000 years ago. The viscous magnetization age of the Sill obtained by Heller and Markert is 818,000 years. This is in fair agreement with the onset of the Brunhes epoch, although there must inevitably be some uncertainty in the effects of possible geomagnetic events occurring during the past 0.7 million years.

QSOs AND GALAXIES

Nature of Ton 256

by our Cosmology Correspondent

THE hypothesis that QSOs are, in general, associated with the nuclei of galaxies (see *Nature*, **242**, 18; 1973) receives a further boost in a recent issue of *Astrophysical Journal Letters* (**181**, L25; 1973). Silk and his colleagues report a detailed study of the nuclear and extranuclear regions of the peculiar object Ton 256. They conclude that the data are consistent with a two component model, where a central QSO-like object is embedded in a giant elliptical galaxy.

Ton 256 is relatively easy to study because of its small (by QSO standards) redshift of 0.131. Spectroscopically, it cannot be distinguished from a QSO but it has a fuzzy photographic image which covers as much as 9 arc s on 200-inch plates. Silk *et al.*, suspecting that "Ton 256 may be a prime example of an object intermediate between a quasar and (compact) galaxy", set out to determine the nature of the fuzz. They chose positions 2 arc s north and 4 arc s southwest of the object's nucleus, and compared the observed properties of the fuzz at those positions with those of the nucleus.

It seems that the fuzz is markedly different from the central object, showing no line emission even though the nucleus has hydrogen Balmer lines and OIII emission. The colours measured for the fuzz also differ from that of the

nucleus and are similar to those of the outer portions of a giant elliptical galaxy.

Silk and his colleagues are able to unravel the probable properties of the two components of the system, assuming that the nucleus of Ton 256 contains a "normal" QSO, and these in no way suggest that the hypothesis is incorrect. The lack of emission lines in the light from the fuzz sets a limit on the amount of gas in the underlying galaxy, again well within the values plausible for a giant elliptical. There seems little doubt that the Ton 256 system is an almost classic example of the QSO embedded in a giant elliptical galaxy and as such will probably be widely referred to as an archetype if that model maintains the remarkable progress towards complete acceptance which it has made this year.

HIGH SPEED PHOTOGRAPHY

Versatile Technique

from a Correspondent

THE host for this year's spring conference of the Association for High Speed Photography, held on April 26 and 27, was the Physics and Chemistry of Solids (PCS) Section of the Cavendish Laboratory, University of Cambridge. The meeting was opened by Dr David Tabor, the present head of the PCS Laboratory, who briefly outlined how interest in high speed photography has developed in Cambridge.

Origin of Cosmic X-ray Background

THE enigma of the cosmic X-ray background seems close to being resolved, after ten years of observation. The sudden increase in the amount of data available, thanks to the success of satellite-borne X-ray instruments, has provided sufficiently stringent limits to make realistic theoretical work possible; previously, so few parameters could be tied down by observation that a wide variety of more or less complex models could be adjusted to fit the data. The flexibility of the situation is highlighted by the fate of the supposed break in the spectrum of this background at 40 keV. A short time ago it was one of the few features which all models had to explain; today, however, there is considerable doubt about whether the break exists at all and it seems that it may well be some kind of instrumental effect.

So where do theories of the X-ray background stand in the light of the latest observations? In next Monday's *Nature Physical Science* (April 30) Fabian relates the Uhuru data on extragalactic sources to the problem of the X-ray background. Radio sources, at least one quasar (3C 273) and clusters

of galaxies are all now known sources of X-rays, and according to Fabian a summation of the contribution of these and as yet unidentified components can explain the background.

With a cutoff brightness for individual sources of 5×10^{-4} counts s^{-1} , a total of 3.4×10^7 sources is required; if the faintest sources are at 3,000 Mpc, corresponding to the Hubble radius, then the mean luminosity of the objects is 9×10^{42} erg s^{-1} . "The important factor," says Fabian, "is that the satellite data give a log N -log S plot agreeing both with the intensity and fluctuations as observed by rockets."

Locating the individual contributors to the background is, however, another problem. Many known sources of extragalactic X-rays have the wrong spectra to fit the bill and Fabian points out that the sought-for objects are probably relatively faint at other than X-ray frequencies. The answer seems to be to concentrate the search on those X-ray source error boxes which are relatively free from bright optical objects, for example those of 2U1443 + 43 and 2U0043 + 32.

In his introductory talk Dr J. E. Field (Cavendish Laboratory) described the laboratory's present high speed camera facilities, which include a 'Fastax', a 'Strobokin', three rotation mirror cameras, capable of microsecond framing rates, and two image converter cameras, the fastest being an 'Imacon' which can give up to twenty pictures at rates up to 10^7 frames a second. These cameras are being used to study many phenomena, including crack propagation, solid and liquid impact, laser irradiation of materials, and the initiation and growth of reaction in explosive materials. The fracture research covers both basic studies and topics of practical interest, such as cleaving of gem stones, the interruption of electric current by fast fracture and fragmentation processes in rock blasting.

Dr M. M. Chaudhri (Cavendish Laboratory) described how he has used high speed microphotography to investigate the detonation of explosives. He has successfully combined microsecond framing rates with magnification onto the film of approximately 10 times and has studied reaction fronts as they propagate through azide crystals, impact loading of explosives, processes occurring when a needle enters an explosive compact and electrical initiation.

Mr S. N. Heavens (Cavendish Laboratory) has used a continuous access rotating mirror camera (AWRE C4 camera) to study the behaviour of thin layers of solids and liquids when impacted in the drop weight test. This method, which involves dropping a weight, typically 5 kg, onto a thin layer, is used extensively for testing the sensitiveness of explosive materials. The advance here is that Mr Heavens has successfully photographed the event using transmission photography.

Dr J. J. Camus and Mr D. A. Gorham (Cavendish Laboratory) described their work on high speed liquid impact. Dr Camus has overcome synchronization problems with the Beckman and Whitley 189 camera by explosively driving a plate target against a suspended droplet. In some experiments the droplet has been formed into a two-dimensional disk of liquid, thus allowing the impact and flow processes to be studied in detail. An interesting result is that gases can be trapped between the impacting surfaces and that when these gas pockets subsequently collapse they cause pressure pulses. This is thought to be important in explaining the onset of damage in materials subjected to repeated low velocity liquid impact (as, for example, in steam turbine erosion). Mr Gorham has looked at the impact of high velocity jets (typically 700 m s^{-1}) on solids using an 'Imacon' camera which allowed him to trigger the camera from the event—a procedure which is impossible with a rotating mirror camera. He gave

several examples to show how high speed drop impact can damage laminate materials.

Three contributions from government laboratories were presented during the conference. Mr D. A. Barnsley (Royal Armament Research and Development Establishment, Fort Halstead, Sevenoaks) reviewed the high speed facilities available at RARDE and Mr J. F. Gilbert (Atomic Weapons Research Establishment, Foulness) and Mr B. G. Houghton (RARDE, Potton Island) described flash, radiographic facilities available in their respective laboratories. The two machines at AWRE, Foulness, are currently operable at up to 3 MV with a maximum stored energy of 60 kJ and an output pulse duration of 60 ns. With a target-to-film distance of 2.4 m reasonable exposure can be achieved after penetration of $\sim 15 \text{ cm}$ in steel. The machine at Potton Island operates at voltages up to 300 kV and gives pulses of duration 100 ns, which are capable of penetrating $\sim 10 \text{ cm}$ of steel.

Both Dr A. W. Palmer (City University) and Dr R. J. Dewhurst (University of Hull) have used 'Imacon' streak

photography—Dr Palmer to study the breakdown of dielectric liquids and Dr Dewhurst to study ultrashort light pulses from an Nd-glass laser: both events need picosecond time resolution. Dr Dewhurst has obtained a resolution of 9 ps using an 'Imacon' 600 with an S1 photocathode (an infrared tube).

Examples of the application of cine-photography were given by Mr A. R. Thompson (University of Sheffield) who has used a camera operating at 3,200 frames a second to study the burning in solid propellant rocket motors. The investigation has yielded valuable information about the factors which affect the stability of the burning. Dr N. Gane and Mr J. A. Williams (Cavendish Laboratory) have used a cin camera at 32 frames a second to study the metal cutting process. In their experimental arrangement the work piece is moved under the tool in a controlled environment. The resulting film shows clearly how different the metal cutting process is under vacuum conditions, with the occurrence of marked adhesion to the tool and an increase in the thickness of the chip.

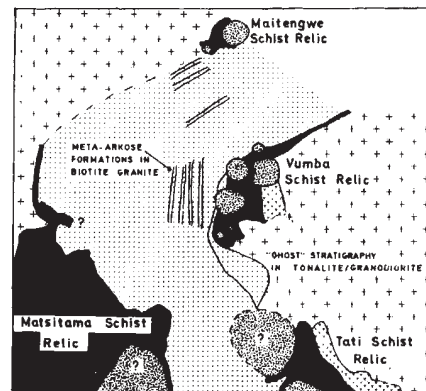
Origin of Archean Schist Relics

In this week's *Nature Physical Science* (April 23), Litherland argues against certain existing and widely accepted models for the origin and evolution of the Archean basement, basing his case on the results of mapping Archean rocks in the Rhodesia Craton. The models in question are those which state or imply that Archean schist (greenstone) belts represent the sites of ancient volcano-sedimentary basins formed, and later deformed, by diapiric behaviour of the underlying primitive crust.

Litherland describes such models as non-uniformitarian in the sense that they take Archean shields to have been formed by the aggregation of separate diapiric granitic nuclei. His own (uniformitarian) conclusion, on the other hand, is that the so-called granitic nuclei are really the end result of a metasomatic process by which schist belts were left as relics of a "folded regional volcano-sedimentary succession in a 'sea' of various granitic rocks". It is for this reason that he refers to the four large schist bodies in the investigated north-east corner of Botswana (see map) as schist relics rather than schist belts.

As a result of his geological studies, Litherland concludes, specifically, that the area investigated was first overlain by a sequence of Archean sediments and volcanics, at which stage the supposedly underlying primitive crust was not visible. Following a deformation, which produced the present pattern of steeply dipping beds, there then occurred the emplacement of circular grano-

diorite intrusions and a widespread regional granitization resulting in large and small schist relics floating in tonalite/granodiorite. In other words, the schist relics are mega-xenoliths of the folded volcano-sedimentary succession rather than sites of isolated deposition, and the granodiorite intrusions represent an early phase in the granitic cycle rather than control of sedimentation and deformation by syntectonic diapirs. Subsequently, a second deformation occurred, imposing the dominant tectonic fabric on the area. Litherland's overriding point is thus that the whole suite of rocks can now be seen to have resulted from lateral accretion rather than from isolated deposition between "granitic nuclei" — a uniformitarian, rather than non-uniformitarian, process.



The suggested geological pattern in north-east Botswana before the second deformation, showing the four large schist relics.

Practical Application of Acupuncture Analgesia

S. B. CHENG & L. K. DING

Chinese Medical Research Centre, 566-568 Nathan Road, Kowloon, Hong Kong

Acupuncture analgesia can sometimes be considered as an alternative to drugs, but it cannot be considered as a universal replacement. There are certain cases, however, where acupuncture analgesia is better than drugs and this technique needs to be developed further in order to assess its true significance. In this article two Chinese doctors, one of whom has conducted twenty-four operations using acupuncture analgesia, assess the advantages and disadvantages of using the technique in surgery.

SINCE the reported success of the use of acupuncture analgesia in several hundred thousand operations in China, great interest in this type of analgesia has been aroused all over the world. In Japan, America and other nations, medical people have begun to investigate the application of acupuncture analgesia in their daily medical and clinical work. This article is an attempt to give some explanation of the technique in order to assist others who are experimenting with it. It is based on the personal experience of one of us who has used this type of analgesia in twenty-four operations—three mitral stenoses, five lobectomies, six gastrectomies and ten thyroidectomies.

Acupuncture analgesia uses no anaesthetic drugs. Instead it is based on Chinese traditional medical theory, *Ching-lo*, which states that by applying pressure to certain specific points on the body, these points (on the meridians) will become numb. Pressure is applied by using a certain method of needling so that pain is either dulled or removed altogether. Thus, in using this technique during surgical operations, sensation in the area in which the operation is to be performed can be dulled while the patient remains entirely conscious.

At present in China this technique is widely used in both urban and rural hospitals and in mobile medical clinics. It has been used for surgery on the brain, neck, chest, abdomen and limbs. In addition, it has been applied in obstetric operations and in operations on the ears, nose and throat. Acupuncture has attained a definite place in Chinese medical practice and, when it can be used appropriately, has become the first choice of anaesthetic.

The use of acupuncture analgesia depends on the availability of properly trained manpower. A skilled medical team including anaesthetists is necessary. The team should be composed both of those who have expertise in the use

of anaesthetic drugs and of those familiar with acupuncture analgesia. Those who have a thorough knowledge of both techniques are obviously the most valuable. Before the operation, one or two members of the team need to visit the patient to find out the patient's mental attitude, his desire to use this kind of analgesia and the nature of his illness. For their part, the team member or members should give a complete explanation of the anaesthesia technique to the patient.

In addition, the team of surgeons must be skilled, alert and capable. The doctors must be those whom the patient trusts, and they must have a thorough understanding of the patient's illness and mental attitude. Before the operation, they must give the patient a complete explanation of the surgical procedure and teach the patient to do the appropriate exercises such as deep breathing. The doctors must work quickly, steadily and accurately and must make the incisions and do suturing in the shortest possible time.

Particular Applications

To illustrate the use of this technique in surgery, we shall describe its application in a thyroidectomy, where the best results have been obtained. Two preliminary steps are necessary. First, the consent of the patient must be obtained. In addition to explaining the procedures to him, it is highly recommended that a demonstration be performed on the patient before his operation. Second, the individuals who administer the analgesia must have a thorough understanding of the procedures. We suggest that the anaesthetists practise acupuncture on their own bodies in order to discover the various methods of using the needle, the different sensations acupuncture produces and the proper depth to which the needle needs to be inserted.

About a half hour before surgery, 50 mg pethidine or other sedative is given to the patient by injection. The acupuncture needles are then inserted at two points on the hand and forearm. The first point is called the *hu ku* and is located between the thumb and the forefinger on each hand. The needle is inserted to a depth of 0.5 inch or until the patient begins to feel sensations of aching, heaviness, fullness and numbness. The other point is the *nei kuan* which is located posteriorly about 2 inches above the wrist. The needle is inserted to a depth of 0.5 to 1 inch, again until similar sensations are felt. If the needles are placed in the proper position and are rotated in a circular manner for about 20 min, an analgesic effect will be induced. The ensuing surgical procedure must be light and fast. When the skin is cut, the technique of "flying knife-rapid cutting" must be used as some pain may be caused when the incision is made.

Acupuncture analgesia does not completely remove pain. For this reason, electric cauterization must not be used to

stop bleeding. Instead, either ligation or pressure must be used. In addition, the patient must be warned that some uncomfortable feeling may be caused when the muscle is pulled around the neck and thyroid gland. The suturing procedure may also cause some pain and must be done as quickly as possible with sharp suture needles. If the level of pain increases, then the needles may be rotated again—or if that fails to bring relief, of course, drug anaesthesia may be resorted to. The whole operation should be performed very quickly to obtain the best results.

Advantages

There are many advantages to the use of acupuncture analgesia. Here we shall discuss three. First, as we have mentioned, acupuncture analgesia, unlike drug anaesthesia, dulls the nerves without causing the patient to lose consciousness; thus, it allows the patient and doctor to co-operate with each other. For example, again in a thyroidectomy, the doctor can talk with the patient at any time, and by listening to the patient's voice can discover if any injury has been caused to the recurrent laryngeal nerve. Another example is found in cases which involve heart and lung surgery. After the opening incision has been made the doctor can direct the patient to do deep abdominal breathing in order to prevent a sudden shift in the mediastinum and to keep the lung inflated so as not to interfere with the operation and ventilation of the patient.

Second, by comparison with the use of anaesthetic drugs, there are fewer physiological and psychological complications. During operations using acupuncture, we have observed that the blood pressure, pulse and breathing remain regular. This is often not the case with drugs. After an operation with acupuncture analgesia, there are no side effects nor evidence of the complications which may follow operations done with drug anaesthesia. Moreover, because there are few physiological reactions, the patient recovers his normal physical and mental state very quickly. In addition, the patient is more psychologically fit after an operation using acupuncture analgesia. According to our observations, the patient has been aware of and understood the entire surgical process as he has been conscious during the operation; therefore, when the operation is finished, he feels the surgery has gone smoothly and has been quite safe. He is very happy with the results. Often, he immediately wants to get off the operating table, walk around and eat.

Third, acupuncture analgesia is more convenient and comparatively cheaper than other known techniques.

Sometimes Inappropriate

One should not assume, however, that acupuncture analgesia is appropriate for all operations or a complete substitute for drug anaesthesia. Again according to our observations, the following factors impose limits on the use of this technique. First, acupuncture analgesia produces different results on different parts of the body. Our past experience has indicated that the results are better if the parts of the body involved are in the chest cavity and above. At present acupuncture analgesia is used in operations involving the head, neck and chest. As mentioned above, the best results have been achieved in thyroidectomies, so this type of surgery is being used for demonstration purposes. Other types of operation in which acupuncture analgesia has achieved good results are lung operations. In surgery on the abdomen and limbs, however, the results have been disappointing.

Second, the surgical cases must be selected carefully. When choosing a patient to undergo surgery with acupuncture analgesia, his illness must be considered. For example, in

operations such as gastrectomies for gastric ulcers and pyloric stenosis, only if the operating time is short is it appropriate to use acupuncture analgesia. But for gastrectomies for stomach cancer in which a rather wide and long investigation is necessary, it is not wise to use acupuncture analgesia. For simple chest surgery such as localized pulmonary tuberculosis, acupuncture analgesia can be used. For exploratory surgery which takes time and in operations for widespread adhesions, it is, however, not advisable to use this technique. In conclusion, at the present time, cases should be chosen in which the nature of the disease is not too complicated and in which the operating time is 1 to 2 h.

Third, the emotional state of the patient must be considered. Because the success of the use of acupuncture analgesia depends on the willingness and understanding of the patient, this is a decisive factor. Before it is used, the entire procedure must be explained to the patient and his active cooperation must be enlisted. Thus, we select those who are emotionally stable, who have a high degree of confidence in the advantages of using acupuncture analgesia, and who are able to follow the doctor's requests and carry out his instructions. This kind of person is usually strong, energetic and young. Unsuitable types of patient include children under 10, who cannot cooperate with the doctor, and highly nervous individuals. In China, because thought preparation is thorough and deep, there are many people who have great faith in the doctors and nurses, and therefore many now volunteer for and even request acupuncture analgesia.

Problems

Acupuncture analgesia in clinical use is still in the initial stages of development. Our experience indicates that several problems have arisen in its application.

Acupuncture analgesia does not dull the nerves as completely and effectively as drugs. Thus, it is not suitable for every operation. On some occasions when the first incision is made, when suturing commences and/or when the procedure is long, the pain threshold may be decreased so as to render the analgesic effect relatively ineffective. It is therefore necessary to have drug anaesthesia available in case it must be used to complete the operative procedure.

Acupuncture analgesia does not cause complete relaxation of the muscles. For example, in abdominal surgery, during the operation the stomach and intestines might be disturbed which could result in a nervous reflex tightening the muscles and hence pain to the patient. If this condition does arise, drug anaesthesia may have to be used.

The theoretical investigation of the effects and uses of acupuncture analgesia is still in the early stages. The results of the use of this technique are only being brought to light through continued experience and experiments. Thus, at present, there is a method of systematic investigation to provide a firm basis for our work. We are now attempting to investigate the relationships between drugs and acupuncture analgesia. Although we believe acupuncture analgesia can be used widely, we feel it is misleading to say that it can and will replace the use of drug anaesthesia. At this stage we can only say that in certain cases acupuncture analgesia is better than drugs. Acupuncture analgesia is only a new anaesthetic technique, and in some contexts at least it may prove to be a better kind of anaesthetic skill. We believe that both acupuncture analgesia and drugs should be used in surgical work in order to further our understanding of the use of this new technique. We admit that it is a radical step to say it is possible merely to insert a needle, in the absence of drugs, to relieve pain. But at a time when the medical field is changing rapidly and new ideas are ever present, the importance of this development must not be neglected.

Features of a Closed-system Economy

R. E. OVERBURY

37 Eamont Court, St John's Wood, London NW8

In a closed-system economy, gross national product is regarded as a "cost of the system". The Leontiev model relating the components of society's demand to resource use has been extended to make possible quantitative estimates of "social entropy" (dereliction, pollution and waste). This kind of work can be further developed, using general activity analysis and programming or optimization methods, to suggest a rationale for minimizing social entropy and real cost in a more advanced economy and technology. But much better thinking about values and objectives is needed for effective progress.

THE basic essentials of a rational economy, conservative environmentally and sensible thermodynamically, are contained in the important notion of "spaceship Earth" or a "closed system", suggested independently in 1966 by Kenneth Boulding of the Institute of Behavioral Sciences of the University of Colorado, and by Barbara Ward (Jackson), now of Columbia University, New York City. Boulding has restated it several times but his approach is perhaps best illustrated in his essay¹ *Economics as an Ecological Science* in which he shows that, historically, the rate of creation of artefacts (knowledge, capital and technology) has outstripped even population expansion, making very high levels of the latter possible before an equilibrium is reached with the crossing of the artefacts and population curves, either by the artefacts becoming hostile to population increase (by pollution, war or exhaustion of resources, for example) or by such overcrowding that disorganization results in failure to innovate or create further wealth.

The choice may be, Boulding suggests, between the "Los Angelization" of the world by postponement of the artefact/population equilibrium point to very high levels, with massive overshoot and catastrophe, or a more gradual running down of resources (increase of entropy). His purpose in several contributions to this discussion is to speculate on the possible nature of a no-growth equilibrium society, to show in economic terms the parallel between our travelling celestial habitat and the situation of the astronauts and to demonstrate the need eventually, if not now, to operate economies and life-support systems with minimum rather than maximum effort and turnover.

As far as I am aware, neither Boulding nor Barbara Ward ever developed their concept, so the problem remains one of making what is now little more than a bright idea more arti-

culate. This article is an attempt at a first step, to suggest further thought, study and criticism. The task, as so often, in developing new approaches is to present well known and familiar facts in a different light.

Closed System

The most important part of Boulding's idea is the recognition of gross national product (GNP) as essentially a cost of the system. In current "open system" economics, consumption and production are generally regarded as "goods in themselves" and GNP measures total throughput from "infinite" sources of exploitable materials to the infinite sinks (both outside the system) but is also viewed as a kind of index of prosperity. In a closed system, throughput is required to be a minimum and effectiveness is measured not by production and consumption as such, but by the "stock" of welfare including capital assets and overall quality of life support systems, including human capital, attainable with the least possible turnover. Any technology able to achieve its objective of improving the quality of life with minimal human effort and use of materials that is at minimum GNP (or, rather, gross national cost) would clearly be a good technology.

It is difficult to persuade economists, committed to "flow" ideas of welfare, to draw an analogy with the physical concept of entropy in the Second Law of Thermodynamics and equate real costs with the increases of entropy in a physical system, and yet a practical analogy with the Apollo systems suggests a closed-system terrestrial economics, merging "econosphere" and ecosystem, with an abatement technology based essentially on recycling and the redesign of products for reduced obsolescence and waste. Current open-system economics could in fact be charged with abstraction in excluding "sources" and "sinks" from its purview.

In *Economics as an Ecological Science*, Boulding is quite specific, and equates the gross consumption of society to real cost in the above sense, and so, according to him,

GNP = gross consumption (real cost) + excess
(used in improving the state of society)

The "excess" term he calls the welfare, state, or condition of society, which is broadly equated with its general capital structure—including human capital, for example education, welfare and life-support systems. Gross consumption includes all other items pertaining to a high level of activity, consumption and disposal. GNP is the gross cost of maintaining society, in terms of payments to the primary factors (the labour, land and capital of elementary economics), to be kept as low as practicable in a closed system, although the distinction between good (excess) and bad (consumption) costs is of course somewhat arbitrary. (The terms good and bad used here and later in this article, although inferred from Boulding's arguments, are not his descriptions but mine.) He suggests that misunderstanding of the real nature of GNP arises because highly developed societies tend to have a high throughput of energy, and energy use is correlated with real income per capita. There is also normally a high correlation between the throughput of production and consumption and the complexity and

elaborateness of society, but this should not lead one to think that the two (gross throughput and real wealth) are the same or that they need always be correlated¹.

Value-added (GNP) Cost of System

The difficulty of seeing GNP as a cost of the system arises, I believe, from the habit (possibly a consequence of accountancy conventions) of viewing it as the value added in economic activity, rather than as the cost of the elementary primary factors, which is the same thing. Yet it should be clear enough that the national income (or GNP, if external trade is omitted) is precisely the amount of the payments made in wages and salaries to labour and management, in profits for entrepreneurial services, in interest and dividends for the use of capital, and in rent, royalties and other sums for natural resources and land. Everyone would presumably agree too that an advanced technology able to provide real wealth with a minimum use of all these resources would be an excellent one, yet its use of primary factors, and therefore the value added, in the above sense, could be quite modest.

I have always found that clarity is helped by reference to the well-known model of an economy in equilibrium developed by Leontiev and his colleagues at Harvard. This model is so familiar to economists and statisticians that only the results required here will be indicated. (The model is described in all modern textbooks of quantitative economics. The treatment here is based on ref. 2.) Essentially it gives the components of the final demand, GNP or national income vector in terms of the gross outputs of industries or sectors of the economy, as well as key technical coefficients representing the input/output pattern of inter-industry flows or state of the technology. In the usual matrix form, it can be written as $Y = AX$ or in full.

$$\begin{pmatrix} Y \\ y_1 \\ \vdots \\ y_i \\ \vdots \\ y_n \end{pmatrix} = \begin{pmatrix} 1 - a_{11} & & & \\ & 1 - a_{12} - a_{22} & & \\ & & \ddots & \\ & & & 1 - a_{nn} \end{pmatrix} \begin{pmatrix} X \\ x_1 \\ \vdots \\ x_i \\ \vdots \\ x_n \end{pmatrix} \quad (1)$$

The column vector Y represents GNP (or national income, omitting external trade); X is the vector of gross sector outputs; $A(a_{ij})$ is the Leontiev or technology matrix. The i th component of the GNP vector, y_i , is the scalar product of the i th row of the technology matrix and the column vector X , so that

$$y_i = x_i - \sum_{j=1}^n a_{ij}x_j \quad (2)$$

for all i . In other words, any component y_i of GNP is the gross output of sector i of the economy minus its sales or inputs to its own and all the other sectors or the amount going to consumer or final demand. Also, assuming linear relations, the general coefficient a_{ij} represents the input from industry i to industry j required to produce unit output of industry j , that is $x_{ij} = a_{ij}x_j$ where x_{ij} is the amount of the product of industry i consumed in producing j units of the product of industry j .

The value of the total production or output of each sector j of the economy, in money terms, is obtained by adding the inputs or purchases from all other industries (column j) to the value added, V_j , or cost of the primary factors, labour, natural resources, enterprise, used by sector j , that is

$$x_j = \sum_i a_{ij}x_j + V_j \quad (3)$$

The total supply by each sector i is the total (intermediate) demand by other industries (row i) plus the final demand by

consumers, y_i , so that

$$x_i = \sum_j a_{ij}x_j + y_i \quad (4)$$

and as, over the whole economy, total production equals total supply or $\sum_i x_i = \sum_j x_j$ we have $\sum_i y_i = \sum_j y_j$ (gross product or value added the same as cost of all payments to primary factors).

Now the general coefficient A_{ij} (equation (1)) of the inverse Leontiev or technology matrix gives the production required by sector i of the economy to meet one unit of demand by component j of the national income (ignoring imports) and the inverse matrix generally gives the pattern of sector gross outputs for any shape of GNP, national income, or "social policy" vector, that is

$$x_i = \sum_{j=1}^n A_{ij}y_j \quad (5)$$

for all i . It is also therefore possible to find the demand created by any component j of GNP on any primary resource simply by multiplying each element of the j th column of the inverse matrix (A_{ij}) by the labour, natural resources, or other factor coefficient f_{hi} for each productive sector x_i , so that the factor cost of the j th component of national income is $\sum_i f_{hi} A_{ij}$ and the total factor use or cost F_h for all components of the national income (omitting external trade) will be

$$F_h = \sum_{j=1}^n \sum_{i=1}^n f_{hi} A_{ij}y_j \quad (6)$$

F_h being evaluated in physical or money terms for any primary factor, such as labour or natural resources, for any pattern of final demand or social policy.

Parallel with Thermodynamics

I believe that from this point it should be practicable to suggest an approximate definition for the social entropy created by economic activity. But care is needed with the analogy as one does not (as Boulding says) have a measure of entropy as neat as that of thermodynamics. Here, however, I take it to include the pollution, including thermal pollution, and waste of industry as well as the whole of the pollution and the currently irrecoverable bric-a-brac of modern society as consumers. A rationale for minimizing social entropy is any framework for deciding how far to pursue recovery and abatement by new technology, although the non-economic or cultural implications of a highly developed closed system are more far reaching.

Allen V. Kneese of Resources for the Future Inc., Washington, has extended the Leontiev model by the addition of pollution generation and abatement activities as further rows and columns of the matrix (A. V. K. and Clifford. S. Russell, personal communication). Using his approach further columns, $n+1, \dots, n+m$, can be added to represent costly abatement activities such as industrial recycling for all the industrial pollution, spoil and waste under the heading of social entropy, as well as m rows to represent their generation, both by the n ordinary industry sectors and as by-products of the abatement technology itself. Thus expanding, say, the l th row ($n < l \leq n+m$) of the enlarged matrix

$$\begin{array}{ccccccc} a_{1l}x_1 + a_{12}x_2 + \dots + a_{1n}x_n + \dots + a_{1n+m}x_{n+m} - x_l & = & y_l \\ \vdots & & \vdots \\ a_{n+m,1} & & -x_{n+m} & = & y_{n+m} \end{array} \quad (7)$$

showing that any such residual (l) generated by each of the n ordinary industry sectors as well as the abatement technology, minus the amount (x_l) by which this item is abated, is the amount reaching final consumers or the environment; y_l is

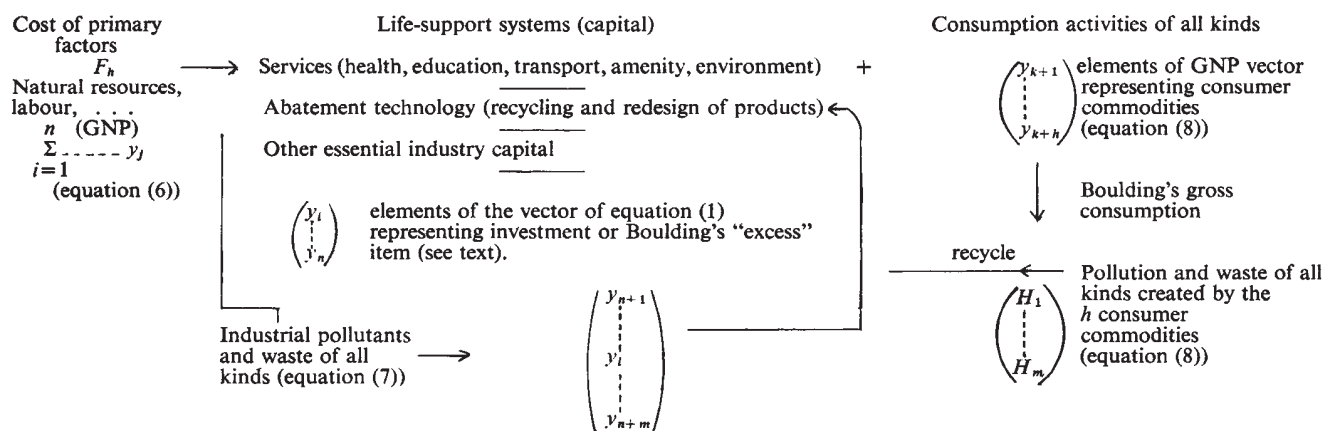


Fig. 1 Pictorial representation of equations (6), (7) and (8). It is not suggested that all consumption activities, which include, for example, food and other essentials and consumption for quality of life, are bad, merely that these are necessarily ephemeral and lead more rapidly to pollution, waste or social entropy. The excess costs represent more permanent structures or negative entropy (see text). Three interesting points arise from this discussion, in my view. (i) There is some current evidence of overcapitalization of industry (see ref. 3) in relation to the chemical industry, and there are undoubtedly other examples. If it can be shown, for example, by the application of equation (5) to a large region, that the gross output required to meet the bill of final demand is less than the technological limit of production, could it not be argued that the excess capacity served only to maintain a state of competition, but no environmental or social purpose? (ii) The MIT model^{4,5} links capitalization directly to pollution, and reduction of resource use only leads to next limitation, a massive pollution crisis⁶. The reasoning of equations (7) and (8) is that capitalization only leads to pollution through the production of polluting industries. Future developments, such as the change from an industrial to a post-industrial or service society, and the effect of large-scale future investments in abatement technology, are likely to increase greatly the capital invested in life-support systems of society, as well, one hopes, reduce pollution and resource use below even current levels. The capital/pollution part of the MIT model therefore needs reformulating. (iii) The analogy of this arrangement of real costs with the Gibbs free energy equation representing the Second Law of Thermodynamics is clear enough.

here regarded as a component of the GNP vector Y (equation (1)), now represented by a column matrix with $n+m$ elements.

The social entropy created by society as consumers (or households, to use the economists' term) can also be represented, following Kneese, by a relation of the form

$$\begin{pmatrix} H_1 \\ \vdots \\ H_m \end{pmatrix} = \begin{pmatrix} -a_{1j} & \dots & -a_{1h} \\ \vdots & \ddots & \vdots \\ a_{mj} & \dots & a_{mh} \end{pmatrix} \begin{pmatrix} y_{k+1} \\ \vdots \\ y_{k+h} \end{pmatrix} \quad (8)$$

where the general coefficient a_{ij} is the amount of pollution or waste i , created for each unit of commodity j consumed ($k+1 \leq j \leq k+h$). And the amount of each pollutant or waste H_i created by consumers is

$$H_i = \sum_{j=1}^h a_{ij} y_{k+j}$$

for all i . The h elements on the right side are the household consumer commodity components of the GNP vector Y (equation (1))*, and the total waste, or social entropy created by the production and consumption activities of society, will be the sum of the vectors†

$$\begin{pmatrix} y_{n+1} \\ \vdots \\ y_{n+m} \end{pmatrix} \text{ and } \begin{pmatrix} H_1 \\ \vdots \\ H_m \end{pmatrix}$$

of equations (7) and (8).

* In other words ($k+1, \dots, k+h$) is a subset of the whole set (n) of all commodities going to final demand (consumers, government, exports and so on). It should be noted also that the coefficients of equations (7) and (8) can be found from industrial engineering and social survey data, an increasing amount of which is becoming available, although more is needed.

† I have here, for convenience, regarded the kinds of pollutants and other waste items ($n+1 \dots n+m$) as identical for industry and for society as consumers. This could be permissible if the range of significant items is chosen to include both, with zero coefficients in the matrices for industry or public as appropriate.

Fig. 1 is a simple pictorial representation of equations (6), (7) and (8), illustrating Boulding's division of GNP (paragraph 6) into the sum of two kinds of costs, namely good costs, applied to bettering the state of society, and the bad costs of simply pushing material through the system to maintain activity and employment—which need not, as he says, be correlated. The good costs amount really to negative entropy, creating highly organized and improbable structures of some permanence, and the bad ones, the rapid transformation of material by industry and general public into more probable and random states. There are no rewards or returns in the system, only the total costs of maintaining society, allocated either to improvement of the assets and infrastructure of a better life, or to consumption, much of it essential and valuable but nevertheless more directly related to running the system down.

Both kinds of cost are represented by equation (6) giving both gross product and its j th component in terms of use of resources (natural resources, labour and so on). The components ($y_{n+1}, \dots, y_{n+m}$) of industrial pollution and waste, and of H (from consumers), also use resources. The distinction between good and bad costs could depend on the time factor and purpose, for example for the set ($k+1, \dots, k+h$) the disposal time is short, but clearly food and home-heating are essential. Nevertheless the wastage of modern society is extremely high and has a tendency to increase still further. On the other hand, investment, especially of the right kind (see Fig. 1), is potentially highly conservative and greatly beneficial to society. Investment in the life-support systems of society is included in the components ($y_1, \dots, y_n$) of national income, excluding the subset ($k+1, \dots, k+h$) representing immediate consumption. The value of the kinds of model described here is that they show the precise relationship between the components of society's demand and the gross outputs of sectors of the economy, including all significant wastes and pollution in terms of technical coefficients (in the matrix transformations) capable of being determined. The coefficients show which components of demand (y_j) make the heaviest call (through sector interdependence) either on polluting industries (equations (5) and (7)) or on especially valuable resources (equation (6)). Most important, they indicate (fairly precisely) the likely impact of detailed process or product redesign on the use of resources, and the possible

contribution of policies to discourage the consumption of commodities, especially in the subset $(k+1, \dots, k+h)$. The Leontiev system does not, however, contain choice of technology within the model. By means of programming concepts one may be able to treat choice systematically to find the best policy for optimal results.

Rationale for Minimizing Social Entropy

Recent work by C. S. Russell and W. Spofford at Resources for the Future Inc. (A. V. Kneese and C. S. Russell, personal communication) has in fact moved from the simpler Leontiev input/output method in the direction of a full programming model, based on more general activity analysis, including industrial activities, recycling and the treatment, transport and disposal of residuals, as well as protective measures within a large region. This is essentially a flexible array of linked systems into which new ecological models (analytical or simulation) of diffusion processes, biological populations, and so on can be fitted as they are developed. Although the Russell-Spofford model is aimed at regional management problems in a conventional open-system economy, this kind of study could, in my view, lead to a more elegant mathematical description of a closed-system economy based on quite practical programming and optimization kinds of thinking. To me the most compelling current feature of the model is the construction of a general activity matrix $(m \times n)$ of m residuals (wastes and pollutants) and n activities ($n > m$) including the activities referred to above. And, as the authors indicate, by-product production and recycling of materials to the input stream are conceptually similar and can be added to the activity columns of the matrix. We have in consequence a fairly major linear programming problem with constraint sets imposed by resource use and other factors. The objective function may, however, be non-linear, involving a series of controlled steps to an optimum solution, by iteration or hill-climbing techniques.

The method also provides at least a measure of our social entropy. In general the authors seek to relate a vector of residual discharges $(x_1, \dots, x_m)$ to ambient concentrations of pollutants or wastes R at M locations in a region, in the form of a vector function $R = Z(X)$, although the form of Z may not even be defined analytically and only discoverable by repeated runs of a simulation model to trace out an ecological response surface. Under very special assumptions (deterministic, steady state models with non-interacting residuals) the authors have suggested that their transfer functions could be constants, and it may be possible to write $R = A(X)$ where A is a matrix of transfer coefficients, a_{ij} , relating the ambient concentration at region location i to a unit discharge of a residual from source j . The damage caused is not the social entropy itself but a measure of it, and if at a location i it is assumed to be a function of the ambient concentration of the k th residual, so that $DM_i = f(R_i^{(k)})$, the measure of social entropy in the whole region would be

$$D_E = \sum_{k=1}^K \sum_{i=1}^M DM_i^{(k)}$$

Differentiation of this equation with respect to each element of the residual discharge vector X gives the marginal damages or shadow prices for use in the objective function of the model. Although for simplicity the Russell-Spofford (RS) model considers the effects at M locations of residuals $(x_1, \dots, x_m)$ from industrial sources, the social entropy created by the general public as consumers and the marginal costs could be included in the model by adding further activity columns to the general matrix to include the transport and disposal of such wastes to recycling centres and so on.

The (RS) model is concerned with minimizing cost in an open-system economy and the activity matrix, although containing a choice of technologies, does not include activities

to reduce the demand, F_h , on valuable resources by the feedback of residuals as inputs of production. Nevertheless, as the authors point out, the additional step would present no conceptual difficulties, and in my view this kind of modelling offers the best hope of practical progress in effective thinking about a closed-system economy. The redesign of products for durability or the recycling of materials could greatly increase the choice of satisfactory, conservative technological solutions lying within the feasible region of the programming model. Ultimately the range of technological possibilities and choices in a closed-system economy could be very wide, as recent work by the Institute of Technology of the University of Minnesota on the advanced systems modelling of an experimental city indicates*. The value of the RS kind of systematic modelling is also that it makes the cost and cost incidence of alternative schemes of management explicit and available to specialized management, regional government, or legislature. It also has a predictive element capable of suggesting the best rational first steps to more advanced, rational and conservative economies and technologies, at any rate on a regional scale.

Values in a Closed-system Economy

The technical argument of this article is that in economic and technological activity considered as a whole there is no profit or reward in an accounting sense, but only the costs of maintaining society (conveniently measured in money). These differ in kind, however, between those incurred in the creation of life-support systems, and educational and other services for quality of life in a post-industrial society, and those related to the throughput of energy and materials merely for high consumption and employment. The latter necessarily degrade the system whatever the time scale, so widely in dispute. I also suggest that a systems approach to economics could offer a wider choice of technologies to abate the demand on resources (F_h of equation (6)) and use them more rationally.

Abatement of the demand on resources means maintaining society and quality of life with less effort, or in plain language not doing everything the hard way. And although the theory of a closed system, merging econosphere and ecosystem, could evolve quite naturally, perhaps along the technical lines suggested by the Russell-Spofford model, the cultural implications of replacing social Darwinism with a systematic economics, nationally and internationally, are so profound that there seems some doubt of its feasibility, at any rate in this era, for it would need an effort of will and imagination from society of an entirely different creative order, of which there is little sign as yet.

Max Weber's well-known sociological study⁹ *The Protestant Ethic and the Spirit of Capitalism* suggests perhaps better than any other how, historically, traditional economics has come to limit horizons and offer society but a single choice—to pay a man to do a job. This is not, of course, to denigrate the monumental contribution of J. M. Keynes to economic theory and practice. This was to show that the monetary system is not part of natural law, but subject to the control of governments, as well as to suggest the technical apparatus of control. But if a man can be paid to do a job which is not really required by society he can, with the same results, be paid not to do it; and Max Weber would have regarded the question as one for Calvinist philosophy rather than economics. Monetary policy has not been touched on here, but it seems probable that in a closed-system economy, Keynes's thinking would still provide a principal means of control. Investment in life-

* The technology of an advanced urban complex is considered in the Minnesota Experimental City Project and related papers, kindly made available to me^{7,8}. This study reviews means of power generation and storage, transportation, use and handling of materials, and design in a closed system. Although some of the technology is not now feasible, the emphasis is on the great potential (assuming some stability of demand), in terms of qualitative benefit as well as economy of resources, likely to be conferred by diversity and range of technological choice, together with logical systems of control attainable with the help of the modelling process.

support systems would probably be large, and multiplier effects considerable unless propensity to consume was reduced by removal of money from the system by taxation or other means. The alternatives of paying a man not to do a job (age of leisure), to do something else or, more generally, to increase the range of choice in life, are technically possible in a systematic economics, for the marginal productivity of labour and probably other resources is almost certainly already zero, and indeed negative for an increasing number of activities. But the objective of increasing the range of choice seems to this writer to be supreme, because it puts the essence of freedom (choice of life mode) before the form of it (political choice).

I have recently made some suggestions about the kind of approach one might hope for in a systems-oriented world¹⁰, but it amounts essentially to putting objectives before organization and replacing the more directionless activities with time for reflexion about rational and human aims for society, as well as some agreement on which problems, scientific and social, are pre-eminently worth solving in this century. As regards the (at present) unquantifiable drop in demand for resources (effort and use of materials) potentially attainable by minimizing throughput, I would not be entirely averse to calling it, following the lead of another member of the meeting at the Royal Institution organized by *Nature* in April 1972, the "hedonistic factor"—although it is probably better to describe it more generally as the opportunity to increase greatly the range and depth of consciousness and awareness and to sharpen rather than dull the senses—in short to avoid one-dimensionality in man.

It is for this last reason too that I believe the authors of the recent *Blueprint*¹¹ may be mistaken as regards aims, because a rational modern society, however it may be constrained by circumstances, could hardly regard a Rousseau-esque existence in villages and forest clearings as an objective, or as anything other than one-dimensional and a system failure. I would hope for a post-industrial, but perhaps even more highly capitalized, society in a very special sense, with a number of key economic and environmental variables, possibly under automatic control—but, and most important, a more relaxed

and appreciative age, aiming at style in thought and performance rather than mere intensity.

The paper on non-renewable resources, presented to the April 1972 meeting at the Royal Institution by Sir Kingsley Dunham, represented the current consumption curve for non-renewable resources as an isolated peak on the line of a very long time scale—a peak possibly to be characterized by our successors as a very curious interval during which the human race experienced a highly intensive but almost entirely one-dimensional life. The value of systems thinking and modelling is that it is a combination of the rational and the intuitive, in thinking about important problems in their entirety. Specialized education and professionalism carry the constant risk of fractional responses and poverty of imagination and approach.

It does not matter if the modelling of important problems is wrong and that the "scrapage rate" for models is high, for it is the process which matters. It is only important to break down disciplinary barriers, and to see that not only economists but scientists, ecologists, engineers and systems men should make a contribution in this area.

Received May 22, 1972; revised February 14, 1973.

- ¹ Boulding, K. E., *Economics as a Science*, Chap. 2 (McGraw-Hill, 1970).
- ² Chenery, H. B., and Clark, P. G., *Inter-industry Economics* (John Wiley, 1965).
- ³ *The Times* (April 20, 1972).
- ⁴ Meadows, D., *The Limits to Growth* (Earth Island Ltd, London, 1972).
- ⁵ Forrester, J. W., *World Dynamics* (Wright Allen Press, Cambridge, Mass., 1971).
- ⁶ Forrester, J. W., *World Dynamics*, 74 (Wright Allen Press, Cambridge, Mass., 1971).
- ⁷ *Minnesota Experimental City*, II (University of Minnesota/Experimental City Project, 1969).
- ⁸ Gilmer, David S., *A Man-made Ecosystem: Some Ideas for Planning New Cities* (Minnesota Project).
- ⁹ Weber, M., *The Protestant Ethic and the Spirit of Capitalism* (Unwin University Books, 1968).
- ¹⁰ Overbury, R. E., *Standard Bank Review* (February 1972).
- ¹¹ *The Ecologist* (January 1972).

Iceland Mantle Plume: Geochemical Study of Reykjanes Ridge

J.-G. SCHILLING

Graduate School of Oceanography, University of Rhode Island, Kingston, RI 02881

Systematic rare earth and minor element concentration variations in basalt recently erupted along the post-glacial Reykjanes Ridge Axis and its northward extension over Iceland reflect the existence, spatial influence, and primordial nature of the Iceland hot mantle plume.

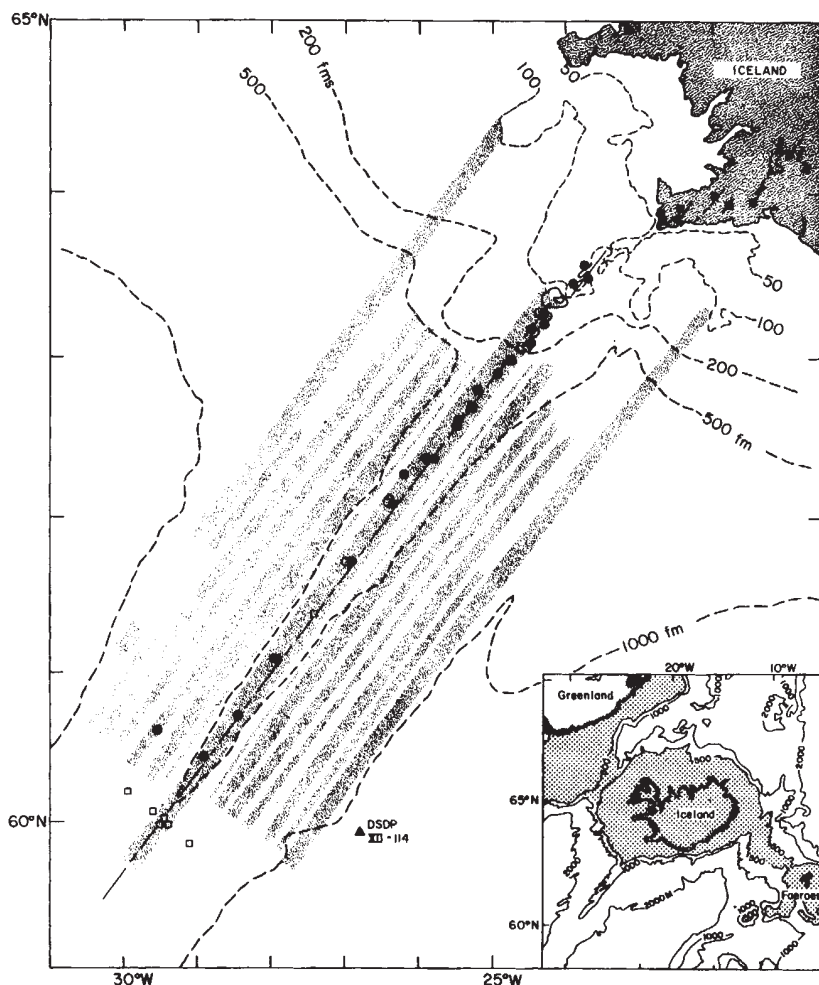
THE Reykjanes Ridge and Iceland (Fig. 1) are key features in North Atlantic and Arctic seafloor spreading¹⁻⁷. It is also in the Iceland Region that the "hotspot" or "hot mantle plume"

concept⁸⁻¹¹ has received support from geophysical evidence on the nature of anomalous mantle beneath the region¹²⁻²¹.

In the Reykjanes Ridge-Iceland region it is possible to follow progressively fissure-type tholeiitic volcanic activity from beneath the sea up on land along the active Mid-Atlantic Ridge. Also, both shield building and fissural tholeiitic volcanism occur on Iceland and can be compared profitably.

Trace elements and Pb and Sr isotopic composition of "tholeiites" from oceanic islands are distinct from those of mid-ocean ridges²²⁻²⁸. These distinctions have been interpreted as reflecting different mantle sources and depth of origin for both types of volcanic activity. Hart²⁹ has stressed the difficulties of maintaining the distinct identity of the two mantle sources when both types of volcanism are closely related spatially and in an area of great mobility, such as for islands straddled on mid-ocean ridges (including Iceland).

Fig. 1 Dredge haul location along the Reykjanes Ridge Axis and neovolcanic zone extension over Iceland. ○, TR101; □, TR41; ■, fissure flow. Shaded strips are normal magnetic anomaly lineaments⁴.



Taking the post glacial Reykjanes Ridge Axis and its extension over the middle neovolcanic zones on Iceland as a "geochemical window into the upper mantle", I now report results which not only infer the presence of a hot mantle plume rising beneath Iceland, but also reveal the extent of its present influence along the Reykjanes Ridge.

Post Glacial Geochemical Variations

During a cruise of RV Trident from July 17 to 19, 1971 (TR101), a nine-thousand-pound suite of fresh pillow basalts and crusts of post glacial age was dredged along the Reykjanes Ridge Axis (Fig. 1); the sampling was extended to historical basaltic flows erupted from known fissures along the middle neovolcanic zone of Iceland.

"Petrographically" the dredged lavas are typically submarine fine grained, vesicular and porphyritic basalts, with plagioclase, clinopyroxene and olivine phenocrysts occurring in variable proportions. The groundmass consists of the same minerals with some opaque minerals. Unlike many mid-ocean ridge tholeiites which are mostly devoid of clinopyroxene phenocrysts or xenocrysts³⁰⁻³¹, they are common in TR101 basalts. Modal abundance measurements show a progressive increase of pyroxene relative to plagioclase phenocrysts toward Iceland³².

"Petrochemically" the basalts are classified as tholeiites according to the Yoder and Tilley normative scheme³². The analyses plot on either side but near the Di-En-Ab plane of silica saturation in the system Di-Fo-Ne-Qz. Most of the Reykjanes Ridge basalts are quartz-tholeiites, except four which are olivine-tholeiites. Four of the subaerial lavas overlap with the submarine lavas, and five others are olivine-tholeiites.

There is a noticeable tendency for the Fe content to increase relative to Mg towards Iceland, and similarly for Na relative to Ca. The $\Sigma\text{FeO}/\text{MgO}$ wt % ratio ranges from 1.2 at 60°N to

1.7 at 63°N, and is accompanied by an increase of the total iron from 10.5 to about 13 wt %. North of 63°N and on Iceland the scatter becomes larger. This is also true for other elements. The SiO_2 content remains remarkably constant along the ridge and its extension over the submerged Iceland Platform, ranging from 49.5 to 50 wt %. It drops markedly over the Reykjanes Peninsula and further north along the middle neovolcanic zone on Iceland (49-47.5 wt %).

"Geochemically," in marked contrast to petrological evidence (which in first order approximation indicates rather uniform tholeiitic basalts), the large incompatible trace elements such as K, La and the minor elements, Ti and P, show striking variations along the Ridge Axis up to Iceland (Fig. 2 and Table 1). These elements decrease regularly and progressively from Iceland where there is a larger scatter down to 61°N, and then level off until the limit of my survey is reached at 60°N. The progressive depletion is a regular function of the ionic radius for the rare earth series (RE). The smallest ion from Gd to Lu remains roughly constant while the light rare earths show more pronounced variations along the ridge axis. La, the largest rare earth ion, shows the greatest variation, and Sm an intermediate one. Thus La/Sm, or related functions such as ratio of enrichment factors relative to chondrites, confirm this point. The $[\text{La}/\text{Sm}]_{\text{E.F.}}$ ratio decreases markedly away from Iceland, possibly in a stepwise fashion (Fig. 3). This ratio is a good indicator of fractionation of the light rare earths and can, with the heavy rare earths, put rigorous limits on the genesis of these basalts³³. Thus, the largest rare earths become increasingly depleted with distance from Iceland to increasingly resemble the so-called "low K_2O , light rare earth depleted submarine ridge tholeiites"^{33,33,34}. At latitude 60°N the light rare earth depletion is more pronounced and the K_2O content lower than on any ridges.

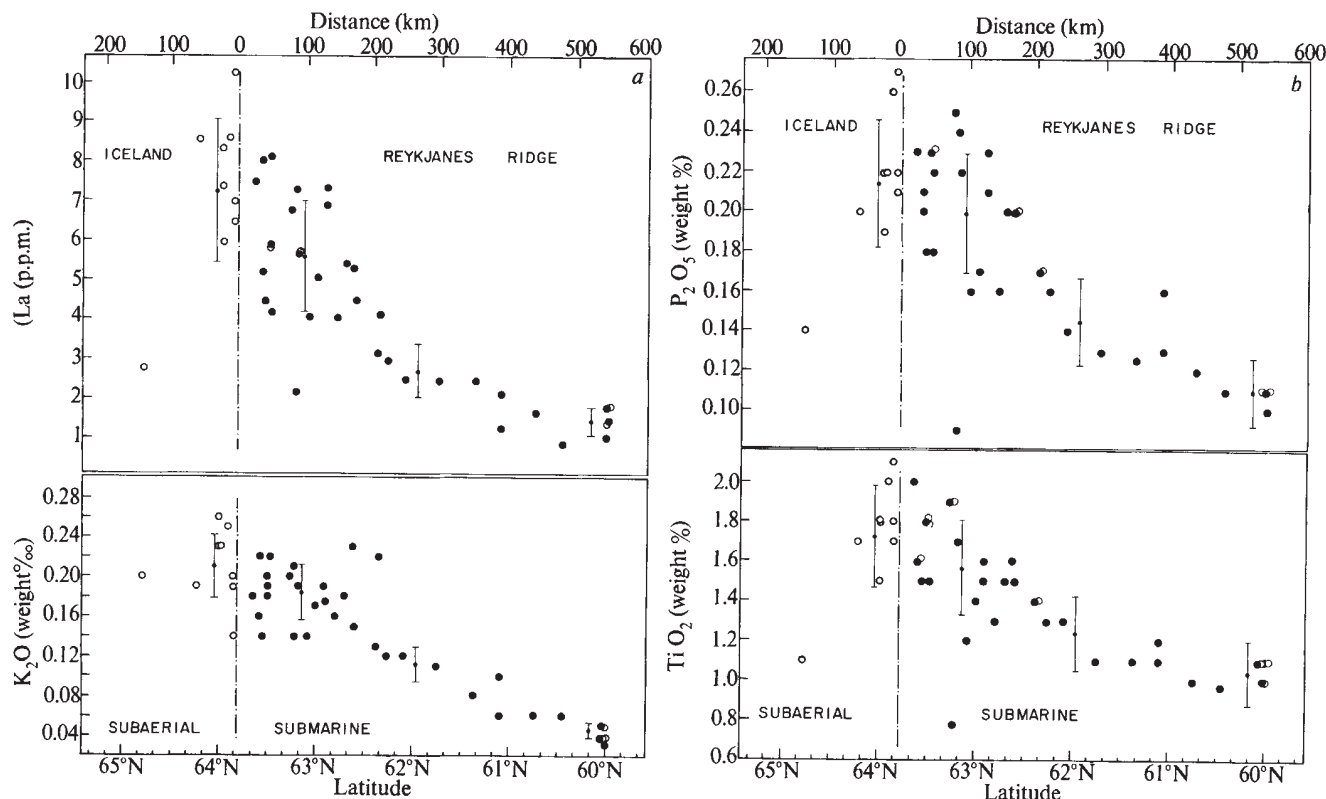


Fig. 2 La, K_2O , P_2O_5 and TiO_2 concentration variations in tholeiitic basalts of Fig. 1. The dispersion error bar was chosen equal to 15% for K_2O , TiO_2 , P_2O_5 and 25% for La, to include analytical and sampling errors and possible post eruptive alterations. This excess scatter, or local noise, invariably increases as the submerged Iceland platform is approached. This corresponds to the region where the ridge is losing its morphological identity and magnetic anomalies II, III and IV become diffused or nonexistent⁴.

The excess scatter over the dispersion error bar shown in Fig. 2 can be attributed to fractional crystallization at shallow depth. Ascents of magmas through a thicker crust³⁵ become more prolonged, intricate, complicate cooling paths at shallow depth, and enhance variations in fractionation.

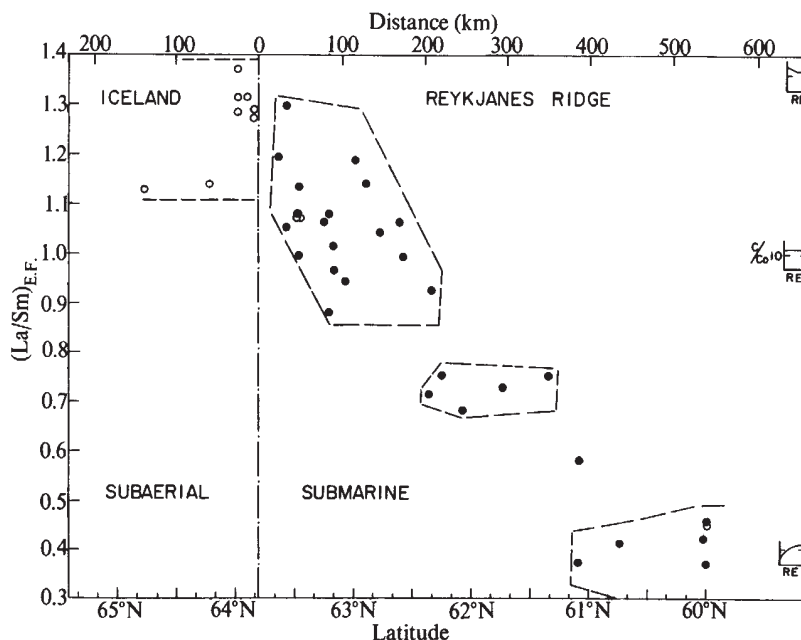
What, then, does the geochemical trend represent? Could this mean that the light rare earth and potassium depletions of the world ridge tholeiites^{23,36} are related to "hot mantle plume" activity, rather than the previously suggested early episodes of

mantle extraction of alkali basalts³⁷ or during continental growth³⁸? Or could the chemical zoning simply represent a transitional zone of mixing between "hotspot" and mid-ocean ridge volcanic activity? Both possibilities recognize the existence of a hot mantle plume beneath Iceland.

Model Requirements

Any model describing the origin of the Iceland-Reykjanes Ridge System should allow for:

Fig. 3 Ratio of La over Sm enrichment factors relative to chondritic meteorites (Table 1) in tholeiitic basalts of Fig. 1. Dashed lines show possible groupings.



The Iceland bulge which is progressively decreasing in height and narrows along the Reykjanes Ridge—reflecting thermal expansion and/or upwelling of the mantle.

Geophysical evidence^{12-15,39,40} suggests partial melting over a wider range of depth beneath Iceland than along the Mid Atlantic Ridge (MAR); from below the Moho to some 250 km.

Crustal thickness which varies from 8–16 km over Iceland³⁵ to 20 km along the Iceland Faeroe Ridge⁴¹ and 3.5–4 km along and across the Reykjanes Ridge⁴. This suggests an unusually large production of magma along the Iceland rift relative to the MAR for the past 50–60 m.y., 2 to 4 times greater than across the Reykjanes Ridge as at 60°N.

V-shaped ridges symmetrically distributed about the ridge axis suggesting a component of mantle flow along the Reykjanes Ridge Axis²⁰.

Regular decrease of K₂O, TiO₂, P₂O₅ (and probably other large ionic lithophile trace elements) away from Iceland along the Reykjanes Ridge Axis.

Small but significant major element zoning.

Modal abundance increase of pyroxene/plagioclase phenocrysts along the Reykjanes Ridge toward Iceland.

The RE patterns progressively changing from slightly light RE enriched near Iceland to strongly depleted southward along the ridge, although the heavy RE stay constant²³.

The widespread and uniformly light RE depleted “tholeiitic ridge basalts”^{23,33,34} dredged far away from any island (or hotspot) interferences as found on the Reykjanes Ridge around 60°N. This suggests a unique and uniform mantle source for these lavas of global extent taken to be the low velocity layer (LVL) in a partially molten state²³.

The low concentration of the large ionic lithophile (LIL) trace elements (K, Rb, Cs, Ba, U, Th, light RE) and radiogenic Pb and Sr isotopes^{26,28} of these ridge basalts, which requires that this mantle source (LVL) be also depleted in these elements. The Sr and Pb isotopes further suggest that the LVL must have undergone such a depletion, by previous episode(s) of melting and magma extraction(s), at some considerably earlier time in the Earth's history^{26,37,42}.

The systematically higher concentration of LIL trace elements and radiogenic Sr and Pb isotopes observed in “oceanic island tholeiites”, including Iceland and other proposed hotspots of tholeiitic composition. This suggests a localized source(s) in the mantle undepleted in these elements, which has remained as a closed system(s) for periods of time long enough to build up the higher concentrations of radiogenic isotopes.

The lack of secular RE pattern variations of tholeiitic basalts erupted at Reykjanes Ridge at 60°N and over the Iceland hot plume, but distinct for the two regions (my unpublished observations). This suggests that production of uniform primary

Table 1 Geochemistry of Reykjanes Ridge Samples

Station	Latitude	Longitude	Depth* (m)	La†	Sm†	Yb†	K ₂ O‡	TiO ₂ ‡	P ₂ O ₅ ‡	[La/Sm] _{E,F}
TR101 1D2	62°35.6'N	25°27.5'W	620-615	4.5	3.1	3.5	0.20	1.50	0.20	0.99
2D1,2	62°37.4'N	25°26'W	650-615	5.3	3.5	3.2	0.23	1.60	0.20	1.07
3D5	62°47.5'N	25°09.6'W	665-575	4.0	2.7	1.9	0.16	1.30	0.16	1.04
5D2	62°54.4'N	24°53'W	555-455	6.9	3.9	3.0	0.20	1.50	0.21	1.25
5D4	62°54.4'N	24°53'W	555-455	7.3	4.5	3.9	0.19	1.60	0.23	1.14
6D9	62°59.7'N	24°41.9'W	450-350	5.0	2.9	2.2	0.17	1.40	0.17	1.19
7D5	63°04.6'N	24°29.8'W	265-365	4.1	3.0	3.1	0.14	1.20	0.16	0.95
11D2	63°16.3'N	24°12.4'W	50-100	6.7	4.4	3.2	0.20	1.90	0.25	1.06
12D7	63°12.9'N	24°16'W	315-200	7.3	4.7	4.6	0.21	1.90	0.24	1.08
12D40	63°12.9'N	24°16'W	315-200	2.2	1.7	1.9	0.14	0.78	0.09	0.88
14D5	63°11.2'N	24°27.6'W	335-355	5.7	4.1	3.2	0.19	1.70	0.22	0.97
15D5	63°34.7'N	23°42.2'W	100-65	5.2	3.4	1.9	0.16	1.60	0.21	1.05
15D18	63°34.7'N	23°42.2'W	100-65	8.0	4.3	2.6	0.22	1.60	0.20	1.30
16D4	63°27.9'N	23°52.4'W	65-55	4.2	2.9	2.5	0.22	1.50	0.18	1.00
17D1	63°28.1'N	23°51.2'W	55-25	5.9	3.8	2.8	0.18	1.80	0.23	1.08
18D1	63°28.1'N	23°51.2'W	46-40	5.8	3.7	2.8	0.19	1.80	0.22	1.08
19D	63°28.2'N	23°48.2'W	140-70	8.1	5.0	4.6	0.20	1.80	0.23	1.14
22D1	62°22.4'N	25°50.7'W	690-740	3.1	3.1	3.3	0.13	1.40	0.17	0.72
23D1	62°21.3'N	25°46.8'W	675-640	4.1	3.1	2.7	0.22	1.40	0.17	0.93
24D6	62°05'N	26°17.8'W	720-653	2.5	2.5	2.1	0.12	1.30	0.14	0.68
27D11	61°44'N	26°52.9'W	605-595	2.5	2.4	3.0	0.11	1.10	0.13	0.73
29D5	61°05.9'N	27°52.7'W	855-765	2.2	2.6	3.7	0.10	1.20	0.13	0.58
30D10	61°05.9'N	27°54.2'W	825-760	1.3	2.4	2.3	0.06	1.10	0.16	0.38
31D9	60°44'N	28°25.4'W	714-705	1.6	2.7	2.6	0.06	1.00	0.12	0.42
33D6	60°27.4'N	28°53.1'W	950-895	0.85	2.0	2.1	0.06	0.97	0.11	0.30
34D6	62°16.1'N	26°08.4'W	550-450	3.0	2.7	3.0	0.12	1.30	0.16	0.76
35D3	62°42'N	25°12.3'W	650-510	5.4	3.4	3.4	0.18	1.50	0.20	1.11
TR41 18D2	59°59.7'N	29°32.5'W	940-1140	1.5	2.2	3.2	0.03	1.10	0.11	0.46
19D	60°00.9'N	29°24.0'W	950	1.4	2.6	3.0	0.04	1.00	0.11	0.37
20D3	60°01.2'N	29°20.8'W	860-1035	1.8	3.0	3.0	0.05	1.10	0.11	0.43
22D1	60°01.2'N	29°28.5'W	930-1025	1.0	2.2	2.6	0.04	1.00	0.10	0.34
38D2	59°59.1'N	29°31.5'W	890-960	1.8	2.8	3.7	0.05	1.10	0.11	0.45
46D1	61°22'N	27°24'W	1625-1720	2.5	2.3	2.1	0.08	1.10	0.14	0.75
IC§ 17	64°47.2'N	20°42.7'W	400	2.8	1.7	0.8	0.20	1.10	0.14	1.13
36	64°12.6'N	21°01.2'W	250	8.5	4.5	3.1	0.19	1.70	0.20	1.14
43	63°59.2'N	21°26'W	300	7.3	3.9	1.9	0.23	1.80	0.22	1.32
47	63°58'N	21°44.5'W	460	8.3	4.2	2.3	0.26	1.80	0.22	1.37
58	63°54.8'N	22°26'W	70	8.6	4.6	2.7	0.25	2.00	0.26	1.32
62	63°58.7'N	21°58'W	140	5.9	3.2	2.2	0.23	1.50	0.19	1.29
HS§ 524	63°51.25'N	22°41.8'W	10	6.4	3.5	2.0	0.14	1.70	0.21	1.27
525	63°50.75'N	22°42.5'W	5	6.9	3.6	1.7	0.20	1.80	0.22	1.33
529	63°51.8'N	22°26'W	80	10.2	5.5	1.4	0.19	2.10	0.27	1.29
Chondrites (Average of 20)				0.30	0.21	0.17				

* Depths for TR101 and TR41 are below sea-level, and for IC and HS are elevations above sea-level.

† Concentration in p.p.m., analysed by an instrumental neutron activation technique.

‡ Concentration in wt %, analysed by L. Shapiro.

§ Iceland samples.

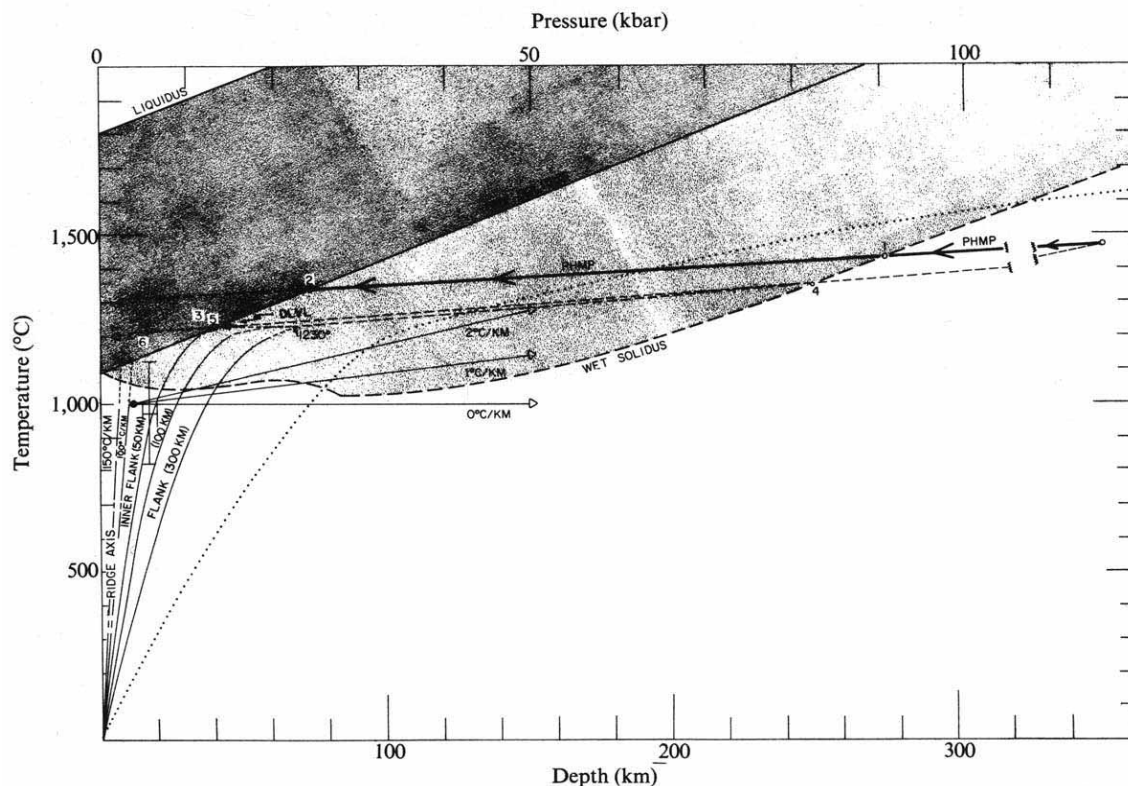


Fig. 4 P, T, isoplethal projection^{49,62} and path for both PHMP and DLVL during ridge injection. Beneath Iceland, 0-1 only PHMP solid rises, 1-2 incipient melting up to 5%, beyond 2 partial melting rapidly increases until the melt begins segregating around 3 or above^{54,55}. Away from hotspot influence, DLVL path is difficult to ascertain because of uncertainties of prevailing geothermal gradients and actual lithosphere thickness. Using the Forsyth and Press thermal model⁶³, partial melting increases rapidly from 5 to 6 beyond which segregation becomes probable^{54,55}. From latent heat due to decompression ($\Delta T \times C_p$) the maximum degree of melting^{63,64} permissible $\gamma = \Delta T C_p / \Delta H_f$ is for PHMP 60%, and for DLVL 33%; more likely values are 20-40% and 10-15% respectively^{64,65,66}. ΔH_f is the heat of fusion. Along transitional zone where mixing occurs, the PT path is indeterminate by this method⁴⁹. Filled circle and error bars are upper mantle temperature and gradient range beneath Iceland inferred from magnetotelluric measurements^{39,40}.

magmas can be maintained for several tens of millions of years over two distinct mantle sources: one light RE depleted for the "low velocity layer", and the other light RE richer for the "hot mantle plume" source. Thus mantle-derived rare earth pattern variations in tholeiitic basalts seem to be functions relatively insensitive to time for certain periods, but rather sensitive functions of position relative to Iceland for the same periods.

The tectonic relations, chemistry, distribution pattern and relative volumes of rock types that are observed in Iceland⁴³⁻⁴⁵. This evidence suggests further magmatic and tectonic complexities beneath Iceland relative to the MAR, enhancing igneous differentiation and fractionation, and anomalous accumulations of melts by stagnation at shallow upper-mantle depths as well as within the Icelandic crust.

Single Mantle Source Models

With these requirements in mind I have explored several dynamical models, in which variation along the ridge of the mineralogy and chemistry of a single mantle source, depth of magma segregation, extent of partial melting and subsequent fractional crystallization during ascent and cooling of the magma occurs, as well as wall rock exchange and/or reaction. None of these single mantle source models can reasonably satisfy all the evidence at the same time.

For example, for lanthanum concentration (Fig. 2) to decrease with distance from the plume, the degree of partial melting would have to increase correspondingly, according to a simple trace element partitioning model I developed^{37,46}, and for any reasonable mantle mineralogy. This is the reverse of what would be expected from crustal thickness consideration^{4,35}, which requires 2 to 4 times greater extent of partial melting beneath the Reykjanes Peninsula. This, of course, assumes a direct relationship between volume of magmas erupted and degree of partial melting in the mantle beneath.

An alternative is to assume the magma beneath Iceland to be melt products of a smaller degree of melting but segregated over a larger volume of mantle (2 to 4 times as much). Melting would extend over a greater depth range and/or over a wider zone, in agreement with other suggestions^{4,12}. But computer rare earth pattern modelling using variable mantle mineralogy and melting conditions indicates that the degree of melting would have to vary between 2 to 30% to satisfy the RE variation observed along the ridge. Production of quartz and olivine tholeiites over such partial melting range is untenable from an experimental petrology viewpoint⁴⁷, as is melting within a volume of mantle 15 times greater per unit ridge length beneath Iceland relative to 60°N.

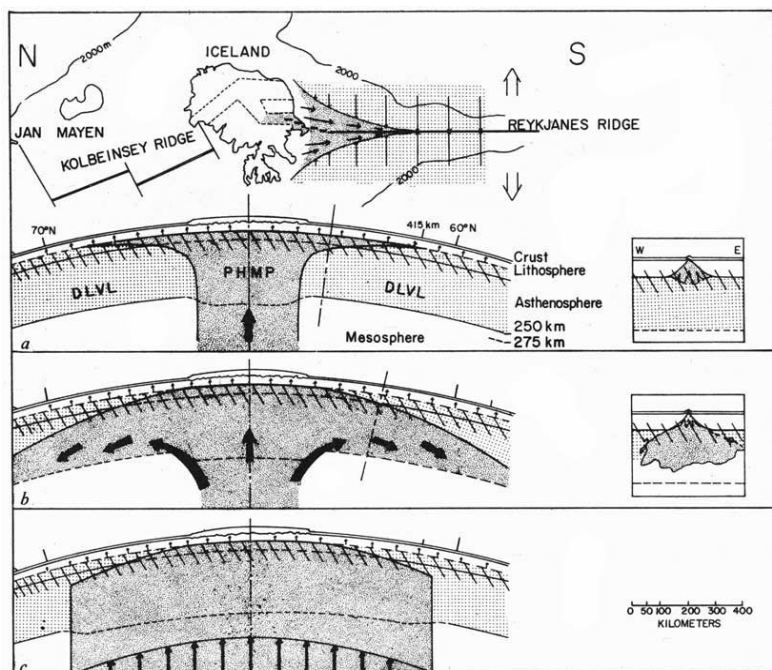
A model assuming successive partial melting and removal of melts^{37,46,48} along the Reykjanes Ridge Axis of a single primordial hot mantle plume flowing vertically beneath Iceland and sub-horizontally about the Iceland plume into the asthenosphere, also seems untenable, although it could be tailored to fit the rare earth pattern variations.

The model must be rejected on Pb and Sr radiogenic isotope evidence. ²⁰⁷Pb/²⁰⁶Pb is known to decrease southwestward along the Reykjanes Ridge up to 60° N (Sun, Tatsumoto and Schilling, unpublished); and similarly ⁸⁷Sr/⁸⁶Sr data available presently at 60°N are lower than on Iceland (Hart, Powell and Schilling, unpublished). This also eliminates the possibility of an increase toward Iceland of the extent of fractional crystallization during the ascent of lava derived from a single source, for explaining the general chemical gradients of Fig. 2.

Hot Mantle Plume Mixing Model

The model satisfying the requirements best is a mixing of melts derived from two mantle sources. These are the primordial hot mantle plume (PHMP) rising beneath Iceland, and

Fig. 5 Schematic flow patterns compatible with observed chemical gradients and mixing model⁴⁹. Hatched zone represents influential depth range where upward diapiric injection can start. *a*, The density of PHMP and flow about the Iceland plume is rapidly used up by feeding the spreading axis. *b*, Showing the PHMP flow progressively sinking about the Iceland plume as a thunderhead¹⁶⁻¹⁸, and the total plume discharge rate is larger than in *a*. *c*, The vertical PHMP flow rate is just sufficient to feed the growing lithosphere beneath Iceland, and its velocity pattern decreases to zero some 400 km away. The plume radius would have to be enormous (600 km) and this is very unlikely. Rheologically, cases *a* and *b* (finger-like flow configurations) may be difficult to realize and to be maintained. The problem may be alleviated considering that mixing may occur by complex flows of porous media into one another, percolation of melts and penetrative convection^{58,67}, because within the last 250 km depth, both PHMP and DLVL mantles are composed of partially molten rocks of slightly different density.



the more passive but more global low velocity layer depleted in large ionic lithophile elements and low in radiogenic Pb and Sr (DLVL). This latter source, usually away from any hotspot influence, feeds the spreading axis with accreting material for new lithosphere and crust generation. The PHMP flow rate is in excess of that required to create new lithosphere beneath Iceland, which is also spreading. During the rise partial melting begins, due to decompression melting⁴⁹ (Fig. 4). The overflow of PHMP is diverted into the low velocity layer (DLVL) either radially¹⁶⁻¹⁸ (Fig. 5) or, more probably, by being preferentially channelled along the Reykjanes Ridge Axis, at the base of the lithosphere, where diapiric injection begins^{20,21} (Fig. 5a). This zone is hotter, mass deficient, and structurally weak because of the spreading. The total Iceland mantle plume discharge is, however, not vigorous enough to feed the Reykjanes spreading axis along its entire length; material from the DLVL will complement laterally. A steady state becomes established. Along the zone of injection and over Iceland, either or both mantles rise, decompress, and partially melt; the two primary melts (and/or the two mantles) mix in proportions which vary along the ridge⁴⁹. South of 60°N the new lithosphere and its upper basaltic crust is derived from the DLVL by passive lateral and upward flows in response to spreading¹⁹; and the melts are feldsparphyric low K tholeiites typical of mid-ocean ridges away from hotspot influences. Near Iceland lava is all derived from the PHMP, and the melts tend to be relatively richer in augite relative to plagioclase phenocrysts. In between is the zone of transition or mixing, where the PHMP contribution decreases regularly southward and the contribution from the DLVL increases.

Although petrologists have usually been reluctant to call for mixing of magmas to explain hybrid melts, their objections seem valid only when the end-member melts are of very different composition, such as rhyolitic and basaltic melts^{50,51}, or when mechanical conditions for mixing are unfavourable. The proposed mixing occurs between two types of basaltic melts of very similar bulk composition. Trace, minor and radioisotopes are indeed quite distinct, as their concentrations depend on the past history of their mantle sources and partial melting conditions, but the difference is insignificant in controlling immiscibility. Mixing of tholeiitic melts along the Kilauea Rift Zone has been convincingly demonstrated⁵², and by analogy the concepts can be applied to the Reykjanes Ridge.

Two important mechanisms are available for the mixing of two component melts.

The first plausible mechanism is by mixing in elongated magma chambers lying beneath the ridge crest, mostly at the base of the crust⁵³. The mechanical mixing occurs in part during segregation, coalescence of interstitial melts and during channelling when both mantle sources upwell and partial melting increases due to decompression (Fig. 4), but mostly in these elongated magma chambers. During diapiric intrusion of crystal-mush, the rate of magma segregation and upward movement is much faster than the residual solid once a threshold of degree of partial melting (ratio of liquid to solid) is reached^{54,55}. Thus melts will tend to accumulate at the base of the crust, ready to feed the ridge axis which is intermittently spreading. There is circumstantial evidence in support of the proposed mechanism, such as the abundant presence of phenocrysts, and resorption and zoning phenomena. But I prefer to defer further discussion until a detailed electron probe study of TR101 collection is completed.

As an alternative hypothesis, the second mixing mechanism I believe plausible is horizontal dike propagation over long distances (>100 km) and parallel to the Reykjanes Ridge Crest volcanic edifice, in a similar fashion to that along the Kilauea Rift Zone⁵⁶. Lava in Hawaii travels sub-horizontally more than 120 km, because of gravity induced symmetrical stress field present within elongated volcanic edifice.

Morphologically, the Reykjanes Ridge, featuring an elevated blocky or triangular central part⁴, seems to be a reasonably close analogue of the Kilauea Rift Zone and its seaward submerged extension. Pillow basalts erupt on both⁵⁷. Further, because of the ridge submergence down to 1 km of water along the transition zone, a horizontal pressure gradient of ~0.5 bar km⁻¹ ridge length should potentially be present along the ridge interior. Upon dike formation and tapping of elongated magma chambers at the base of the crust, there should be a tendency for PHMP derived lava to rise diagonally along the ridge with a significant southwestward horizontal component of velocity. Lava derived further away from Iceland, ultimately from the DLVL, will also rise diagonally, but less so, as the southwestward horizontal component of velocity decreases southwestward with submergence. Thus the path of both component melts cross, allowing mixing to occur in a similar way as described by Elder's experiments⁵⁸, and as petrologically documented by Wright and Fiske⁵² for hybrid lavas erupting along the Kilauea Rift Zone in Hawaii. These two mechanisms need not be mutually exclusive, but may operate simultaneously, thus enhancing the chances of mixing.

Flow Patterns about Mantle Plumes

Although I have been able to ascertain the extent of the Iceland hot plume influence along the Reykjanes Ridge by using its axis as a window into the upper mantle, the geochemical approach does not allow me to distinguish between patterns of flow (Fig. 5), vertically or horizontally, nor between radial and pipe-like asthenospheric flows^{16-18,20}.

What can be ascertained is that the influence of the PHMP flow on the zone of magmatic injection must decrease progressively and die out some 400 km southwest from the tip of the Reykjanes Peninsula. This is more than 3 plume radii from the centre of the plume, assuming the plume to cover Iceland (radius = 200 km). Formulation of the mixing model and further references are given elsewhere⁴⁹.

Ridge Morphology

An intuitive approach to determine shallow asthenospheric flow is to consider the ridge morphology. Iceland and other hotspots represent significant bulges on the geoid, interpreted from gravity to reflect rising currents in the mantle^{16,17}. The funnel shaped morphology of the Reykjanes Ridge suggests a forceful axial flow of PHMP from Iceland southwestward, perhaps with some piling east and west of Iceland. South of 60°N along the narrow part of the funnel ridge shape, only DLVL material feeds the ridge in response to spreading (Fig. 5). North of Iceland, the axial asthenospheric PHMP flow is probably more complex due to the proximity of the Greenland Continent which is underlined by a relatively cold upper mantle, and perhaps also influenced by the Kolbeinsey Fracture Zone. Thus a steady state flow pattern was more difficult to reach north of Iceland. This is reflected by more subdued morphology of the MAR, and several recorded jumps of the position of the active rift zone during the past 60 m.y. (ref. 59).

I suggest that the seafloor morphology over and around a hotspot region reflects the shallow asthenospheric flow of PHMP about the "thunderhead". A three directional starlike flow, more intense along the two MAR branches, would be predicted for the Azores hot plume, and similarly for the Afar hotspot⁶⁰.

It seems that at shallow asthenospheric depth, flows of PHMP material around plumes tend to be preferentially channelled along crustal weakness as ridges, and the intensity of flow along these directions is controlled in part by their spreading rates and prevailing stress and thermal fields. This does not preclude some PHMP radial transport beneath the plates from the hot plume centre. The intensity of such "radial" flows will, however, be comparatively small. From mass continuity principles one can easily imagine complex counterflows of the more passive DLVL material toward sections of the spreading ridges far removed from any hotspot centre, for example, south or 60°N along the narrow elevated part of the ridge.

Contrary to earlier views⁶¹, the model implies that plumes are transporting to the Earth's surface a more primordial mantle material(s) than present in the low velocity layer lying beneath those mid-ocean ridge segments remote from plume. The upper few hundred kilometres of the Earth's mantle would therefore have been drained of large ionic lithophile elements and low-melting fractions during geological time to generate continents; the more primordial mantle richer in these elements and volatiles would lie deeper in the Earth, accumulating with time radiogenic isotopes and heat, thus leading to upward convection.

I thank D. Gottfried, M. D. Tapia, M. Zajac and M. Osti for help; F. deMeglio and his staff for neutron irradiations and facilities at RINSC; Professor N. D. Watkins for review; T. W. Johnston and other students; and Captain Hansen and crew of RV Trident. This work has been supported by the US Office of Naval Research and the National Science Foundation.

Received January 16, 1973.

- ¹ Bodvarsson, G., and Walker, G. P. L., *Geophys. J. Roy. Astron. Soc.*, **8**, 285 (1964).
- ² Heirtzler, J. R., LePichon, X., and Baron, J. G., *Deep-Sea Res.*, **13**, 427 (1966).
- ³ Vine, F. J., *Science*, **154**, 1405 (1966).
- ⁴ Talwani, M., Windisch, C. C., and Langseth, jun., G., *J. Geophys. Res.*, **76**, 473 (1971).
- ⁵ de Boer, J., Schilling, J.-G., and Krause, D. C., *Earth Planet. Sci. Lett.*, **9**, 55 (1970).
- ⁶ Decker, R. W., Einarsson, P., and Mohr, P. A., *Science*, **173**, 530 (1971).
- ⁷ Krause, D. C., and Schilling, J.-G., *Nature*, **224**, 791 (1969).
- ⁸ Wilson, J. T., *Nature*, **197**, 536 (1963).
- ⁹ Wilson, J. T., *Can. J. Phys.*, **41**, 863 (1963).
- ¹⁰ Wilson, J. F., *Nature*, **198**, 925 (1963).
- ¹¹ Wilson, J. T., *Phil. Trans. Roy. Soc.*, **258**, 145 (1965).
- ¹² Bott, M. H. P., *Geophys. J. Roy. Astron. Soc.*, **9**, 275 (1965).
- ¹³ Tryggvason, E., *Bull. Seism. Soc. Amer.*, **54**, 727 (1964).
- ¹⁴ Francis, T. J. G., *Geophys. J. Roy. Astron. Soc.*, **17**, 507 (1969).
- ¹⁵ Long, R. E., and Mitchell, *Geophys. J. Roy. Astron. Soc.*, **20**, 41 (1970).
- ¹⁶ Morgan, W. J., *Nature*, **230**, 42 (1971).
- ¹⁷ Morgan, W. J., *Bull. Amer. Assoc. Pet. Geol.*, **56**, 203 (1972).
- ¹⁸ Morgan, W. J., *Mem. Geol. Soc. Amer.* (in the press).
- ¹⁹ Isacks, B., Oliver, J., and Sykes, L. R., *J. Geophys. Res.*, **73**, 5855 (1968).
- ²⁰ Vogt, P. R., *Earth Planet. Sci. Lett.*, **13**, 153 (1971).
- ²¹ Vogt, P. R., *Nature*, **240**, 338 (1972).
- ²² Hubbard, N. J., *Earth Planet. Sci. Lett.*, **5**, 346 (1969).
- ²³ Schilling, J.-G., *Phil. Trans. Roy. Soc.*, **A268**, 663 (1971).
- ²⁴ Gast, P. W., *Phys. Earth Planet. Int.*, **3**, 246 (1970).
- ²⁵ Tatsumoto, M., *J. Geophys. Res.*, **71**, 1721 (1966).
- ²⁶ Tatsumoto, M., *Science*, **153**, 1094 (1966).
- ²⁷ Welke, H., Moorbath, S., Cumming, G. L., and Sigurdsson, H., *Earth Planet. Sci. Lett.*, **4**, 221 (1968).
- ²⁸ Peterman, Z. E., and Hedge, C. E., *Geol. Soc. Amer. Bull.*, **82**, 493 (1971).
- ²⁹ Hart, S. R., *Carnegie Institution Wash., Year Book*, **70**, 353 (1971).
- ³⁰ Aumento, F., *Can. J. Earth Sci.*, **5**, 1 (1968).
- ³¹ Melson, W. G., and Thompson, G., *Phil. Trans. Roy. Soc.*, **A268**, 423 (1971).
- ³² Yoder, H. S., and Tilley, C. E., *J. Petrol.*, **3**, 343 (1962).
- ³³ Frey, F. A., and Haskin, L. A., *J. Geophys. Res.*, **69**, 775 (1964).
- ³⁴ Kay, R., Hubbard, N. J., and Gast, P. W., *J. Geophys. Res.*, **75**, 1585 (1970).
- ³⁵ Palmason, G., *Visindafelag Iseldinga*, **XL**, 187 (Reykjavik, 1971).
- ³⁶ Engel, A. E., Engel, C. G., and Havens, R. G., *Bull. Geol. Soc. Amer.*, **76**, 719 (1965).
- ³⁷ Gast, P. W., *Geochim. Cosmochim. Acta*, **32**, 1057 (1968).
- ³⁸ Philpotts, J. A., and Schnetzler, C. C., *Can. Mineral.*, **10**, 375 (1970).
- ³⁹ Hermance, J. F., and Grillo, L. R., *J. Geophys. Res.*, **75**, 6582 (1970).
- ⁴⁰ Hermance, J. F., and Grillo, L. R., *Phys. Earth Planet. Int.* (in the press).
- ⁴¹ Bott, M. H. P., Browitt, C. W. A., and Stacey, A. P., *Marine Geophys. Res.*, **1**, 328 (1971).
- ⁴² Hart, S. R., and Brooks, C., *Carnegie Institution Wash. Year Book*, **68**, 426 (1970).
- ⁴³ Jakobsson, S. P., *Lithos*, **5**, 365 (1972).
- ⁴⁴ Ward, P. L., *Geol. Soc. Amer. Bull.*, **82**, 2991 (1971).
- ⁴⁵ Rutten, M. G., *Marine Geophys. Res.*, **1**, 235 (1971).
- ⁴⁶ Schilling, J.-G., and Winchester, J. W., in *Mantles of the Earth and Terrestrial Planets* (edit. by Runcorn, S. K.), 267 (Interscience, London, 1967).
- ⁴⁷ Green, D. H., *Phil. Trans. Roy. Soc.*, **A**, **268**, 707 (1971).
- ⁴⁸ Shaw, D. M., *Geochim. Cosmochim. Acta*, **34**, 237 (1970).
- ⁴⁹ Schilling, J.-G., *J. Geophys. Res.* (in the press).
- ⁵⁰ Yoder, H. S., *Carnegie Institution Wash. Year Book*, **69**, 141 (1970).
- ⁵¹ Yoder, H. S., *Carnegie Institution Wash. Year Book*, **70**, 105 (1971).
- ⁵² Wright, T. L., and Fiske, R. S., *J. Petrol.*, **12**, 65 (1971).
- ⁵³ Cann, J. R., *Nature*, **226**, 928 (1970).
- ⁵⁴ Green, D., and Ringwood, A., *Contr. Min. Petrol.*, **15**, 103 (1967).
- ⁵⁵ Sleep, N. H., thesis, MIT (1972).
- ⁵⁶ Fiske, R. S., and Jackson, E. D., *Proc. Roy. Soc.*, **329**, 299 (1972).
- ⁵⁷ Moore, J. G., and Reed, R. K., *US Geol. Surv. Prof. Paper*, **475B**, 153 (1963).
- ⁵⁸ Elder, J. W., *Liverpool Geol. Soc. Special Issue*, **2**, 245 (1970).
- ⁵⁹ Johnson, G. L., Vogt, P. R., and Avery, O. E., in *Geology of the East Continental Margin* (edit. by Delany, F. M.), **70/14**, 53 (Institute of Geological Sciences, London, 1971).
- ⁶⁰ Schilling, J.-G., *Nature Physical Science*, **242**, 2 (1973).
- ⁶¹ Birch, F., *Geol. Soc. Amer. Bull.*, **76**, 133 (1965).
- ⁶² Yllie, P. J., *J. Geophys. Res.*, **76**, 1328 (1971).
- ⁶³ Forsyth, D. W., and Press, F., *J. Geophys. Res.*, **76**, 7963 (1971).
- ⁶⁴ Bott, M. H. P., *Geophys. J. Roy. Astron. Soc.*, **14**, 413 (1967).
- ⁶⁵ Uffen, R. J., and Jessop, A. M., *Bull. Volc.*, **26**, 59 (1963).
- ⁶⁶ McConnell, R. K., McClaine, L. A., Lee, D. W., Aronson, J. R., and Allen, R. V., *Rev. Geophys.*, **5**, 121 (1967).
- ⁶⁷ Ramberg, H., *Lithos*, **4**, 259 (1971).

Archaeological Occurrences of Early Pleistocene Age from the Shungura Formation, Lower Omo Valley, Ethiopia

H. V. MERRICK

Department of Anthropology, University of California, Berkeley

J. DE HEINZELIN

Geologisch Instituut, Rijksuniversiteit—Gent, Gent

P. HAESAERTS

Geografisch Instituut, Vrije Universiteit te Brussel, Brussels

F. C. HOWELL

Department of Anthropology, University of California, Berkeley

Stone artefacts have been excavated from stratified contexts in the upper members of the Shungura Formation. One occupation site and several secondarily-derived concentrations of artefacts were found. Potassium-argon age determinations for these occurrences indicate an age of approximately 2.0 million years. The artefacts recovered offer a contrast to those known from Lower Pleistocene deposits at Olduvai Gorge, Tanzania, and from still older deposits east of Lake Rudolf in Kenya.

ARCHAEOLOGICAL sites have been found stratified in Lower Pleistocene deposits in the Omo Valley. This is an important addition to the limited number of Plio-Pleistocene localities which have yielded artefacts, fauna, hominid remains and radiometrically datable materials. The only other notable occurrences in this time range are those of Olduvai Gorge, Bed I¹ and east of Lake Rudolf, Kenya².

Stratified concentrations of artefacts in derived contexts were first located in the Shungura Formation in 1971 by two of us (J. de H. and F. C. H.). These occurrences, designated FtJi1 and FtJi3³, are in fossil localities 204 and 208 within Member F (previously localities 204, 206–208 were assigned to Member E⁴). Subsequently a programme of excavation and intensive survey undertaken (by H. V. M.) in 1971 and 1972 led to the location of a possible occupation site (FtJi2) in locality 396 in Member F. This report summarizes: (1) the stratigraphy and geochronology of known archaeological occurrences in the Shungura Formation; (2) the specific features of the two sites so far excavated; and (3) the characteristics of the artefact assemblages.

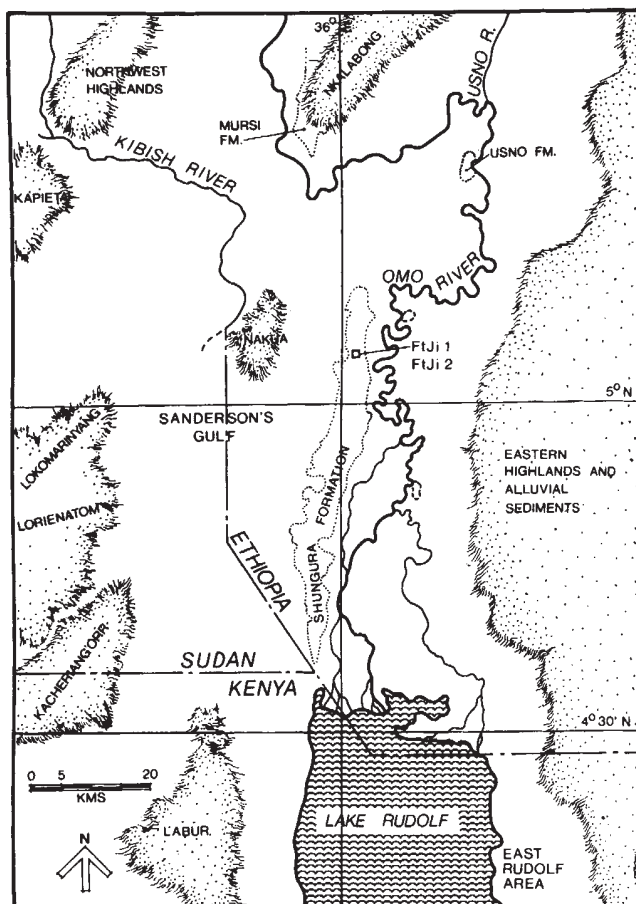


Fig. 1 Generalized map of the lower Omo basin, southern Ethiopia, indicating the locations of Plio-Pleistocene exposures. Highlands are stippled.

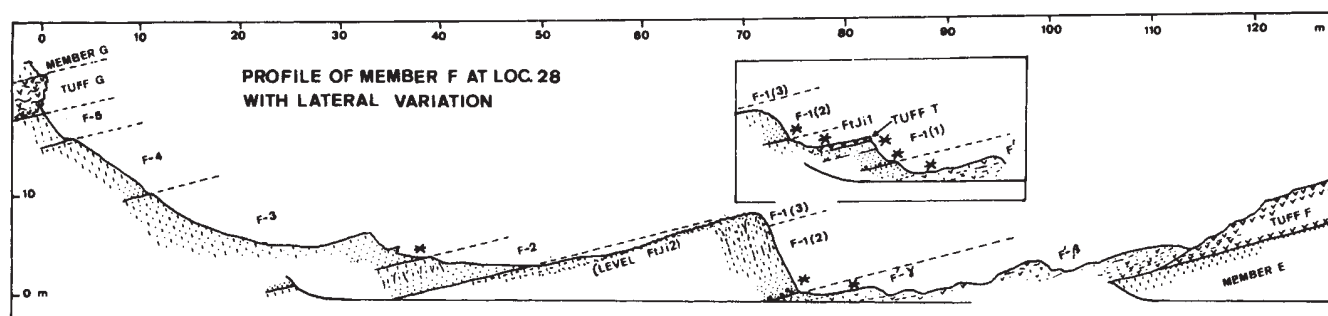


Fig. 2 Profile of Member F at locality 28, near the FtJi2 occurrence. The inset profile indicates the relative stratigraphical position of the FtJi1 occurrence.

Archaeological Occurrences

The Shungura Formation has an aggregate thickness of more than 700 m and a surface area of exposure of about 200 km². A series of K/Ar age determinations suggests that the base of the formation is older than 3.75 m.y.⁴, and the top is somewhat younger than 1.4 m.y. (personal communication from F. H. Brown). The formation has been divided into ten members on the basis of nine widespread volcanic tuffs, designated Tuffs A through J. Each member, except the Basal Member, is composed of a principal volcanic tuff and the overlying series of sediments up to the base of the next principal tuff. The sediments of Members A through lower G are primarily fluvial, and the sediments of middle Member G and above are generally prodeltaic and lacustrine.

The lower members, Basal Member through Member E (roughly 4 to 2 m.y. old), have not yet yielded stone artefacts or occupational surfaces *in situ* in completely unambiguous situations. J. Chavaillon has reported the finding of a chopper in an outcrop of Member E, but subsequent excavation did not reveal any associated concentration of other artefacts⁵. Other occurrences of artefacts, none of which is definitely attributable to occupational surfaces, were found between 1968 and 1971 by two of us (J. de H. and F. C. H.) in sediments and tuffs of Member D and in channel situations in Member C⁶. At these occurrences the artefacts were found on the surface in situations which suggest that they originated from Shungura Formation deposits, but positive proof of this origin is lacking.

Thus far Member F deposits contain the oldest stone artefacts which are definitely *in situ* and the oldest artefacts associated with occupational surfaces. Occurrences in Member F have been found in two separate areas approximately 2 km apart in the northern outcrops and two other sites have recently been located in the southern outcrops (personal communication from J. Chavaillon). Isolated *in situ* artefacts have also been excavated from channel gravels in lower Member G, and other occurrences still to be studied have been found eroded from this member. No archaeological occurrences have been located in the uppermost lacustrine members, H, I and J, of the Shungura Formation.

Stratigraphy and Geochronology

Member F has a measured thickness of some 35 m of fluvial and floodplain sediments. Tuff F, at the base of the member, usually varies between 4.0 and 4.5 m but may attain a maximum thickness of 7 m. The average of two K/Ar age determinations for Tuff F gives an age of 2.04 ± 0.10 m.y.⁴. Overlying Tuff F are four to five cyclic units of fluvial and floodplain sedimentation. Each unit is usually composed of a graded sequence of coarse sands passing upwards through medium and fine sands to silts and clays. Occasional weakly developed palaeosols occur on the top of some of the silt and clay subunits within the individual cyclic units. The lowest unit in Member F, F-1, contains a widespread minor tuff, Tuff T. Fig. 2 illustrates the nature and variation of the sedimentary sequence near the FtJi2 occurrence and the relative stratigraphical positions of

the FtJi1 and FtJi2 occurrences within Member F. Tuff G which directly overlies Member F has yielded a K/Ar age determination of 1.93 ± 0.10 m.y.⁴.

The FtJi2 Occurrence

The FtJi2 occurrence in locality 396 appears to be a primary context occupation site. The site is presently being dissected by the erosion of a small spur of fluvial sediments in fossil locality 396. Thus far a small excavation of 2 × 5 m has recovered ninety-five artefacts *in situ*. The stratigraphical sequence at this locality is summarized in Fig. 3. The occurrence is a low density concentration of small artefacts scattered in a lens 12 to 15 cm thick, situated about 130 cm above the base of subunit F-1(3) (Fig. 3). The artefacts, almost entirely small fragments of shattered quartz lumps and pebbles, are the only stone fragments in the entire 2.5 m thickness of clay forming the lower half of subunit F-1(3). The artefact-bearing horizon has not yielded bone at this locality, but fossil pollen is preserved (personal communication from R. Bonnefille). The excavation has not yet determined the areal extent of the occurrence. The surface scatter of artefacts along the adjacent erosion exposure suggests a maximum north-south extension of about 20 m, but the east-west extent of the occurrence cannot yet be determined.

That the initial concentration of the artefacts was the result of hominid activity and that the occurrence can be considered an occupation surface is suggested for the following reasons. First, the artefacts occur in a very localized vertical and horizontal concentration in a clay deposit in which stone fragments and pebbles are otherwise totally lacking. Their physical condition is invariably fresh so that the possibility that these artefacts could have been selectively transported, concentrated and deposited in these sediments can be eliminated. Second, quartz lumps, small cobbles and their shattered fragments are generally extremely rare in even the coarsest gravels of the Member F deposits. This suggests that quartz is an exotic raw material introduced into the deposits from sources at least several kilometres distant. Thus only hominid activity

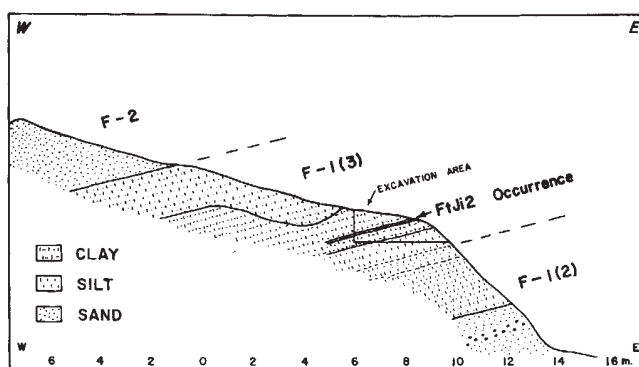
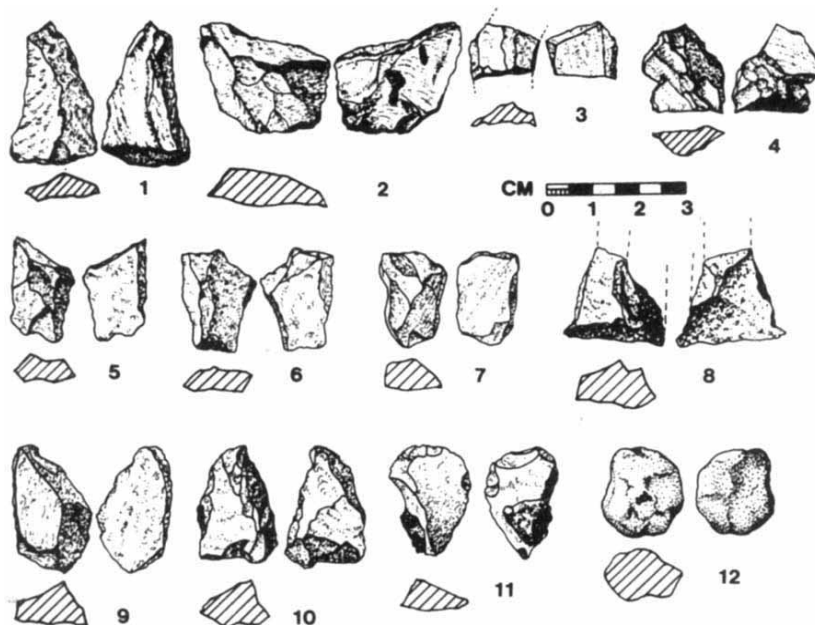


Fig. 3 Profile of Member F at locality 396 and the FtJi2 occurrence.

Fig. 4 *In situ* quartz artefacts from the FtJi2 occurrence. Flake fragments (1–2), angular fragments (3–11), and small pebble (manuport) (12).



can feasibly account for the localized presence of the quartz artefacts in the fine clay sediments.

It is not yet possible to reconstruct the detailed environmental setting of this occurrence. Stratigraphical studies (by J. de H. and P. H.) suggest, however, that initially the artefacts were scattered on a temporary land surface of silty clay in a backswamp or in a marginal flood basin situation. Subsequently, continued deposition under backswamp conditions buried the site. As a result of both the secondary development of a calcium carbonate concretionary horizon and the location of the site in clays which are subject to marked swelling and shrinkage with wetting and drying, all traces of the original temporary land surface have been obliterated. Undoubtedly this has slightly disturbed the scatter of artefacts and may account in part for their vertical dispersion.

The FtJi1 Occurrence

Fig. 5 shows the distribution of surface artefacts near fossil localities 204–208 and 215. There are six small ($\pm 25 \text{ km}^2$) patches of densely concentrated artefacts in the $500 \times 100 \text{ m}$ area included in these localities. Furthermore, the entire locality 206–207 area is covered by a low density scatter of artefacts. These surface occurrences are almost entirely small quartz artefacts. The position of the artefacts at the base and

on the steep slopes of freshly eroded exposures makes it almost certain that most of them were recently eroded from Member F sediments. The absence of exposures of the Kibish Formation (of late or post-Pleistocene age) overlying the Shungura Formation in this area largely excludes the possibility that these discrete patches of artefacts could derive from geologically recent sediments. Moreover, each occurrence is associated with eroded exposures of a small stream channel which has been shown by excavation in locality 204 (the FtJi1 occurrence) to contain artefacts *in situ*. It is highly probable therefore that most of the artefacts in these surface scatters are of Member F age.

The FtJi1 occurrence in locality 204 was tested by an excavation of $2 \times 4 \text{ m}$. A total of 130 artefacts and probable artefacts was recovered *in situ* from a small lens of sands and gravels, approximately 30 cm thick, filling the base of a small meandering channel. This channel is stratigraphically some 6 to 8 m below the FtJi2 occurrence and is situated at the base of the F-1(2) subunit (Fig. 2, inset profile). Mammalian fossils, including elephant, hippopotamus and several species of bovids, were also found. The artefacts appear to be in a secondarily-derived context; they were found in the gravel lenses of the channel fill and their physical condition varies from fresh to heavily abraded. The fossils also appear to be in a derived context, for they vary in condition from unrolled to

Fig. 5 Location of FtJi1 and FtJi3 occurrences and surface artefact scatters in the locality 204–208 and 215 area. Locality boundaries are shown with dashed lines.

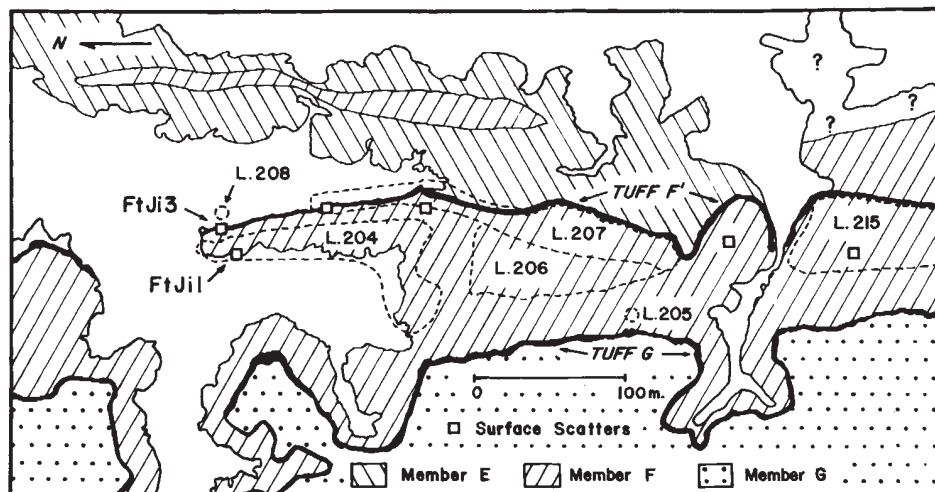


Table 1 Artefacts Recovered in the FtJi1 and FtJi2 Excavations

Artefact category	FtJi1 Excavated—1971			Surface (quartz only)		FtJi2 Excavated		Surface	
	Quartz	Lava	Chert	1971	1972	1971	1972	1971	1972
Debitage									
Flakes	11	—	—	17	2	1	2	2	3
Flake fragments	4	1	—	29	1	—	2	10	3
Angular fragments	107	1	6	197	11	11	77	52	23
Manuports	—	—	—	—	—	—	2	1	—
Total artefacts	122	2	6	257	—	95	—	94	—
Range of maximum length (mm)	7–39	19–55	9–17	10–64	—	5–32	—	8–40	—
Mean maximum length (mm)	16.0	—	12.7	24.8	—	13.4	—	20.4	—
Distribution of maximum length (mm)	%					%			
1–5	—					1.1			
6–10	18.9					38.9			
11–15	38.0					30.5			
16–20	23.7					12.6			
21–25	13.1					10.5			
26–30	4.9					4.2			
31–35	2.5					2.1			
36–40	0.8					—			
41–45	—					—			

heavily rolled. This suggests that both artefacts and fossils were concentrated in and deposited as part of the bed load of the stream. As numerous channel fills in Member F contain abundant mammalian fossils but lack artefacts, the presence of both artefacts and fossils in this channel may be fortuitous. Any postulated association between the artefacts and fossils, due to hominid activity before their incorporation into the channel deposits, is problematical.

Stratigraphical studies are inconclusive as to the original surface(s) from which the artefacts are derived. They may have been derived from primary contexts on the channel banks and flood basins adjacent to the small channel. The FtJi2 occurrence would be an example of a primary context site in a similar situation. It is also possible that the artefacts may have been preferentially discarded on the sandy substratum of the dry stream channel and subsequently reworked into the bed load of the stream. A pattern of preferential tool discard in channel situations such as this has been noted for the early sites east of Lake Rudolf².

Artefact Assemblages

The artefacts recovered in the FtJi1 and FtJi2 excavations and from the adjacent surface scatters are recorded in Table 1. A selection of the excavated artefacts from FtJi2 is illustrated in Fig. 4. The artefacts recovered from both sites are predominantly of milky white vein quartz. Most of them are small angular fragments of shattered pebbles and lumps of stone. Large core tools, such as choppers, and large artificially introduced unmodified natural stones (manuports) are not present in any of the excavated or surface samples. The size of the artefacts recovered in both excavations is small (see Table 1). The mean maximum length of quartz artefacts from FtJi1 is slightly longer (16 mm to 13.4 mm) than that of FtJi2. The frequency distribution of maximum length indicates that there are significantly fewer very small artefacts at FtJi1. This may be attributable in part to size sorting by stream action during deposition at FtJi1.

Most of the quartz artefacts from both occurrences are fragments of shattered quartz lumps. Whole flakes and flake fragments (pieces with a part of the bulb of percussion remaining) are rare. Pieces exhibiting intensive secondary retouch are absent. Several excavated pieces from FtJi2, however, show minor traces of edge damage, perhaps caused by utilization. These quartz artefacts can probably be interpreted best as the debris resulting from the intensive smashing of small quartz lumps and pebbles.

In the FtJi1 excavated sample there are two lava artefacts—one a split flake and the other an angular fragment. There are also six possible chert artefacts. All six are very small angular fragments and are slightly abraded. Small chert pebbles occur with moderate frequency in the stream channel deposits so that it is possible that these were produced naturally.

The artefact density of 9.5 artefacts per square metre on the FtJi2 occupation surface is low compared with most of the Lower Pleistocene occurrences at Olduvai Gorge¹. It is fairly comparable to the low densities of artefacts recorded from the early sites east of Lake Rudolf², particularly because the ideal depositional conditions at the FtJi2 occurrence and the recovery techniques used allow the recovery of minute fragments which increase the apparent density of artefacts.

By comparison with the early archaeological occurrences known from Olduvai Gorge and from east of Lake Rudolf, the most notable features of the Omo occurrences are the absence of large tools, such as choppers, the preponderance of quartz as a raw material, and the generally smaller size of the artefacts. Although part of these differences may relate to different activity facies, at least part may be explained by the nature and proximity of the available raw material. Member F sediments are almost totally lacking in suitable raw material for tool manufacture. A tentative palaeogeographical reconstruction of the lower Omo basin for this time suggests that the principal outcrops of lava and quartz may have been 20 to 30 km distant. The nearest source of raw material probably would have been in stream channels several kilometres distant. These channels, draining the highlands forming the eastern margin of the valley, would have provided only small quartz lumps and pebbles for raw material.

These field studies have been supported by grants-in-aid to one of us (F. C. H.) from the National Science Foundation. We gratefully acknowledge the continued support and encouragement of the Government of Ethiopia and thank the Kenyan Government for its continued cooperation.

Received January 4; revised February 16, 1973.

¹ Leakey, M. D., *Olduvai Gorge*, 3 (Cambridge University Press, 1971).

² Isaac, G. L., Leakey, R. E. F., and Behrensmeier, A. K., *Science*, 173, 1129 (1971).

³ Nelson, C. M., *Bulletin No. 4 of the Commission on Nomenclature and Terminology*, Pan-African Congress on Prehistory and the Study of the Quaternary (1971).

⁴ Brown, F. H., and Lajoie, K. R., *Nature*, 229, 483 (1971).

⁵ Chavaillon, J., *Bull. Soc. Prehist. Fr.*, 67, 7 (1970).

⁶ Howell, F. C., in *Calibration of Hominoid Evolution* (edit. by Bishop, W. W., and Miller, J. A.), 331 (Scottish Academic Press, 1972).

Genetic Apparatus of *Stylonychia* sp.

D. M. PRESCOTT, K. G. MURTI & C. J. BOSTOCK*

Department of Molecular, Cellular, and Developmental Biology, University of Colorado, Boulder, Colorado 80302

The course of DNA changes during the formation of a macronucleus from a micronucleus in *Stylonychia* is followed.

Stylonychia sp. is a common ciliated protozoan with a genetic apparatus that makes it unusually useful for the study of the structure and function of the eukaryotic chromosome. *Stylonychia* is a hypotrichous ciliate, and preliminary evidence indicates that the phenomena described here hold for all hypotrichs, for example *Euplotes*.

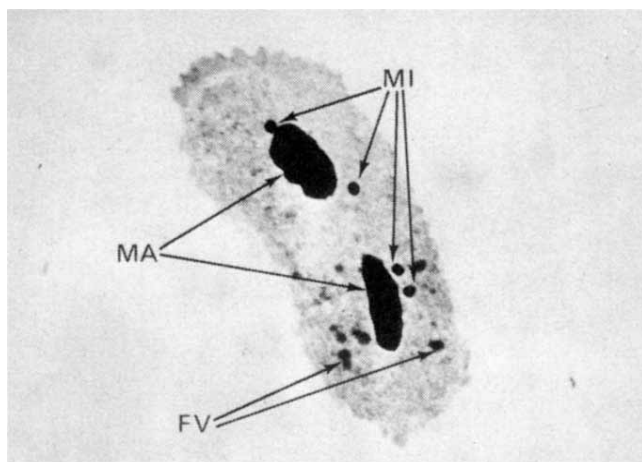


Fig. 1 A *Stylonychia* stained with the Feulgen technique and with fast green. The two macronuclei (MA) and four micronuclei (MI) are visible. The cell contains a few food vacuoles (FV) that stain with fast green.

Each *Stylonychia* cell contains four genetically identical, diploid micronuclei and two macronuclei that together contain about 600 times more DNA than a single diploid micronucleus (Fig. 1). Conjugation follows essentially the same plan as in other ciliates. The micronuclei of each conjugant undergo meiosis to form haploid nuclei. The cells exchange a haploid nucleus, and the exchanged nucleus fuses with a resident haploid nucleus to form a new diploid micronucleus. During this process, the old macronuclei and remaining haploid micronuclei are destroyed. The new micronucleus divides twice, producing four diploid micronuclei. Two of the nuclei remain as micronuclei, one is destroyed, and the fourth, the macronuclear anlage, is converted into a new macronucleus. Finally, the two micronuclei and the newly formed macronucleus divide in the absence of cytokinesis to produce the nuclear constitution of the vegetative cell.

The development of the macronuclear anlage is accomplished by a sequence of changes in the DNA. Ammermann¹

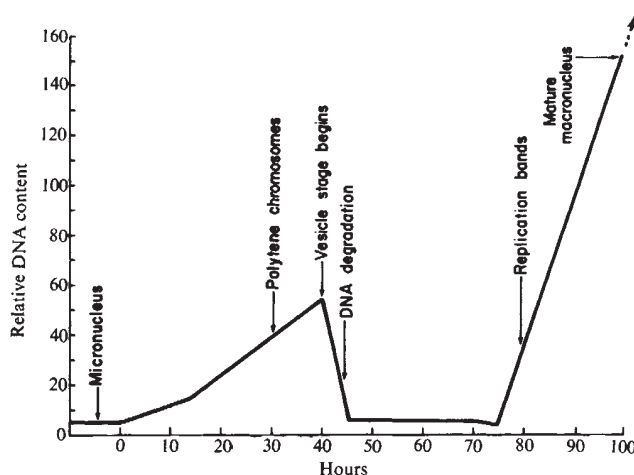


Fig. 2 The course of DNA changes during the development of the macronucleus from a micronucleus. The initial DNA build-up results in the formation of polytene chromosomes. The polytene chromosomes are cut into individual bands, and most of the DNA is then degraded. Finally, the remaining DNA replicates many times to produce the mature macronucleus. Modified from Ammermann² with permission.

has described the course of DNA increase that occurs as a micronucleus is converted to a macronucleus. These DNA changes are shown in Fig. 2, originally published by Ammermann². The micronucleus that becomes the macronuclear anlage increases its DNA content approximately sixteen-fold, and in the course of this increase polytene-like chromosomes are formed. Soon after their formation, the polytene-like chromosomes are destroyed, beginning with the appearance of membraneous partitions between all the bands or chromomeres (Fig. 3a and b)³. The process continues such that each chromomere band becomes completely enclosed in a physically independent membrane-bound vesicle¹ (Fig. 3c). In summary, the macronuclear anlage is converted from a bag of polytene-like chromosomes to a bag of thousands of vesicles, each containing the material from a single band and parts of the two adjacent interbands. Nothing is known about the mechanism by which the chromosomes are cut up into separate bands or chromomeres. The transectioning occurs in the interband regions and at some stage presumably involves a double stranded endonuclease, the action of which is restricted to the interband DNA.

The splitting up of the chromosomes is followed by a reduction of 93% of the DNA to acid-soluble products, probably nucleotides⁴. After the destruction of DNA the vesicles disappear, and the remaining 7% of the DNA is more or less evenly distributed throughout the macronuclear anlage. A few hours later this remaining DNA goes through many rounds of replication, each taking place by means of the replication bands that are characteristic of hypotrichous ciliates. The DNA content increases to approximately 600 times the amount

* Present address: Clinical and Population Cytogenetics Unit, Medical Research Council, Edinburgh.

BOOK REVIEW SUPPLEMENT

Biomedical Book Reviewing

CHING-CHIH CHEN

School of Library Science, Simmons College, Boston

THE rapid proliferation of publications in the biomedical fields creates a great need for a constant, systematic, and critical selection of current biomedical literature. Because biomedical research is multi-disciplinary, book reviews have become primary tools in the selection and evaluation of biomedical titles. Reviews "which throw light on a work from the various interlocking fields can point the way from an indiscriminate mass of scientific writings to scientific literature. When critical judgments are pooled and receive bibliographic recognition over a sustained period, it becomes apparent that such collective criticism tends to sift and to lift out the works that are substantial, articulate and mature¹".

For the most part, book reviews appear in subject journals, so in order to determine the current status of biomedical reviewing, I conducted a study of 285 general biomedical journals held by the Science Library at the Massachusetts Institute of Technology. Fifty-four English journals, which include *bona fide* book reviews, were selected. Detailed statistics were taken of books reviewed in all 1970 issues of these fifty-four journals. The book titles were checked against the "Weekly Record" section of *Publishers' Weekly*, *Forthcoming Books* and other pertinent sources to determine precise publication dates. Here I present a summary of those findings which seem to be of greatest interest to biomedical and other scientific researchers. No attempt is made in this article to describe the methodology of the study. Papers describing and discussing the project in much greater detail are planned for later publication in a library journal. It is possible to identify the principal biomedical book reviewing journals in terms of their quantitative coverage of reviews, point out the duplication patterns in book reviewing among these sources, and explore the effectiveness of these media in terms of speed of reviewing, comprehensiveness of review treatment, and authority.

The statistical results of this study of biomedical book reviewing reveal that there were 3,347 reviews of 2,067 biomedical books in the fifty-four journals in 1970 (Table 1). A reasonable approximation of the annual biomedical book output (including imports) in 1969-70 in the United States is about 2,300 titles (ref. 2 and C. C. C., in preparation), so the statistics suggest that quite a high proportion of these books (approximately 70%) were reviewed at least once in the fifty-four journals studied.

Table 1 also reveals that many books originally published in Britain and reviewed in the media (77.4%) were either reprinted or distributed in the United States by American publishers. Williams and Wilkins of Baltimore seems to be most active in this context. The 1,674 books available in the United States and reviewed in the media were either published or reprinted and distributed by 161 American publishers and/or distributors. The top three publishers alone accounted for 443 titles (26.46%) of the 1,674 books. They are, in decreasing order of activity, Williams and Wilkins of Baltimore, C. C. Thomas of Springfield, Illinois, and Academic Press of New York.

Fourteen journals accounted for 72.3% of all the reviews

Table 1 Statistical Summary of Biomedical Book Reviews Appearing in the Fifty-four Journals Examined for 1970

No. of reviews	No. of books
2,393	Of 1,370 books originally published in the United States
604	Of 354 books originally published in Great Britain (274 of these were either reprinted or distributed in the United States)
350	Of 343 books originally published in countries other than USA and Great Britain (mostly in languages other than English) (30 were either reprinted or distributed in the United States)
Total 3,347	Of 2,067 books (1,674 of which were available in the United States)

that appeared in the fifty-four periodicals examined for 1970. Details of the extent of review coverage in these journals will be found in Table 2.

Of the 2,067 titles reviewed in the fifty-four journals in 1970, 727 (35.17%) were reviewed in more than one journal. Of these, 420 were reviewed twice, 162 three times, and the rest (145 titles) four to eleven times. These 727 titles accounted for 2,007 reviews (59.96% of the total of 3,347 reviews). Only three of the fifty-four periodicals studied did not contain any reviews of these 727 books. Although these 2,007 reviews are scattered among almost all journal titles studied, the fourteen journals listed in Table 2, column 2, proved most likely to contain reviews of frequently reviewed titles, accounting for 1,576 (or 78.52%) of the 2,007 reviews of the 727 titles.

A random check of every third title of the 727 books was made to determine the duplication pattern in book reviewing among journals that have a tendency to review titles also reviewed elsewhere. The following interesting overlap results were obtained: about 35%—*Brit. Med. J.* and *Lancet*; 30%—*Ann. Intern. Med.* and *Arch. Intern. Med.*; 16%—*Lancet* and *Ann. Intern. Med.*; 15%—*J. Amer. Med. Assoc.* and *Lancet*, *J. Amer. Med. Assoc.* and *Brit. Med. J.*, *J. Amer. Med. Assoc.* and *Ann. Intern. Med.*, *J. Amer. Med. Assoc.* and *New Engl. J. Med.*

In the field of medicine, as in other areas of science and technology, currency of information is particularly crucial to researchers. "The success of any book selection tool would be in direct ratio to the time lapse between the publication of the book and its evaluation"³. The present study shows that the time lags among the fifty-four journals varied very widely, ranging from a mean of 5.8 months after publication for *Lancet* to 42 months for *Acta Radiol. Ther.* A rank list of the fifty-four periodicals studied, in ascending order of average time lag, was compiled. Some journals, such as *J. Dairy Sci.* and *Canad. J. Genet. Cytol.*, although relatively speedy in reviewing, are insignificant as selection media because of their small quantitative review coverage of the literature. Therefore, the list of highest ranked journals in terms of speed of reviewing

Table 2 Rank List of the Top Fourteen Biomedical Reviewing Journals in Terms of *X*, *Y*, and *Z*

<i>X</i> (Quantitative coverage: No. of book reviews)		<i>Y</i> (Quantitative coverage: No. of reviews of the 727 books reviewed more than once in 1970)		<i>Z</i> † (Time lag)	
Title	No. of reviews	Title	No. of reviews	Title	Mean time lag (months)
(1) * <i>Brit. Med. J.</i>	375	* <i>Brit. Med. J.</i>	244	* <i>Lancet</i>	5.8
(2) * <i>Lancet</i>	317	* <i>Ann. Intern. Med.</i>	215	* <i>Brit. Med. J.</i>	6.6
(3) * <i>Ann. Intern. Med.</i>	277	* <i>Lancet</i>	204	* <i>J. Amer. Med. Assoc.</i>	7.2
(4) * <i>J. Amer. Med. Assoc.</i>	260	* <i>J. Amer. Med. Assoc.</i>	168	<i>Brit. J. Haematol.</i>	7.2
(5) * <i>Arch. Intern. Med.</i>	211	* <i>Arch. Intern. Med.</i>	138	<i>J. Bone Joint Surg. (GB)</i>	8.6
(6) * <i>New Engl. J. Med.</i>	174	* <i>New Engl. J. Med.</i>	121	* <i>New Engl. J. Med.</i>	8.8
(7) <i>Quart. Rev. Biol.</i>	155	<i>Quart. Rev. Biol.</i>	80	<i>Clin. Pharmacol. Ther.</i>	8.9
(8) <i>Bioscience</i>	123	* <i>Canad. Med. Assoc. J.</i>	72	* <i>Yale J. Biol. Med.</i>	9.3
(9) * <i>Canad. Med. Assoc. J.</i>	116	* <i>Arch. Pathol.</i>	65	* <i>Ann. Intern. Med.</i>	9.6
(10) <i>Amer. J. Med. Sci.</i>	102	<i>Amer. J. Med. Sci.</i>	63	* <i>Canad. Med. Assoc. J.</i>	9.8
(11) * <i>Gastroenterology</i>	91	* <i>Gastroenterology</i>	59	* <i>Arch. Pathol.</i>	10.2
(12) * <i>Arch. Pathol.</i>	89	<i>Bioscience</i>	55	* <i>Radiology</i>	10.2
(13) * <i>Yale J. Biol. Med.</i>	67	* <i>Yale J. Biol. Med.</i>	52	* <i>Gastroenterology</i>	10.4
(14) * <i>Radiology</i>	63	* <i>Radiology</i>	40	<i>Brit. J. Radiol.</i>	10.5
Total 2,420 (72.3% of the grand total of 3,347 reviews)		Total: 1,576 (78.52% of the 2,007 re- views of the 727 books)			

* Titles appear in all three columns. † Journals containing fewer than twenty reviews in 1970 were ignored.

(Table 2, column 3) includes no journal that reviewed fewer than twenty books in 1970. It is encouraging to find that several periodicals rank high in terms both of coverage and speed of reviewing as indicated in Table 2.

Detailed statistics of time lags for all 3,347 reviews were also collected (Fig. 1). Clearly there is a very wide range of time lags in biomedical book reviewing, ranging from reviews coinciding with the date a book came off the press (0 month) to that appearing 108 months (9 yr) after publication. The highest number of reviews appeared around 8 months after the books with which they dealt were published. Simple calculation yields a mean time lag of 10.43 months ($\bar{T}_1$) for all 3,347 reviews and a standard deviation of 6.63 months (σ). This means that a review of a biomedical book will most probably (68% confidence) be found within the time limit imposed by 17.06 months $< T_1 < 3.8$ months.

I made no attempt to study the quality of each book review. Nevertheless, the average review length (number of words) of each journal was computed. This can be a fair indication of the degree of comprehensiveness of review treatment in each journal. It was generally found that journals with longer time lags in reviewing tended to be the more scholarly periodicals and to provide longer reviews (with an average length as long as 865 words). The highest ranking fourteen journals from the standpoints of coverage and promptness were found

to include much shorter reviews (with an average length of about 350 words).

Of the fifty-four journals studied, only three titles, *Lancet*, *Brit. J. Haematol.* and *Canad. Med. Assoc. J.*, do not have signed reviews, and all these appeared among the highest ranking fourteen journals for coverage and promptness of reviewing.

It is obvious from this study that many biomedical reviewing journals can be disregarded by researchers and librarians for selection purposes because of the limited number of titles covered by their reviews and their comparative sluggishness. On the other hand, one can easily identify from Table 2 those reviewing media most likely to be useful for selection.

Considering that many scientific books are already out of date before they are published, it should be stressed here that an unreasonable time lag in reviewing is a very serious problem in terms of book selection and evaluation. This study showed that although many reviews meet high standards of criticism and evaluation, they are of little value in selection because they do not appear promptly enough. Earlier studies have shown that, on the average, book use declines most rapidly in the early years following publication—about one third of a scientific book's usage is in its first year after publication and half in its first three years⁴. If therefore a library book order were generated from a review which had a time lag of 6 months, library readers would not be able to use that book until at least 9 months or a year after its publication. One third of this book's life would then have passed.

It seems reasonable to suggest that if reviews are to be useful for medical book selection, the time lag between the publication date of a book and the appearance of a review ought not to exceed 3 or 4 months. At present, all journals studied clearly are far from meeting this modest goal. It is, however, one toward which all editors of journals, reviewers, book publishers, and librarians and researchers who use the reviews should work collectively and diligently.

I thank Arthuree Wright for assistance in collecting the review data, and Thomas J. Galvin and Robert Cheshier for corrections and comments.

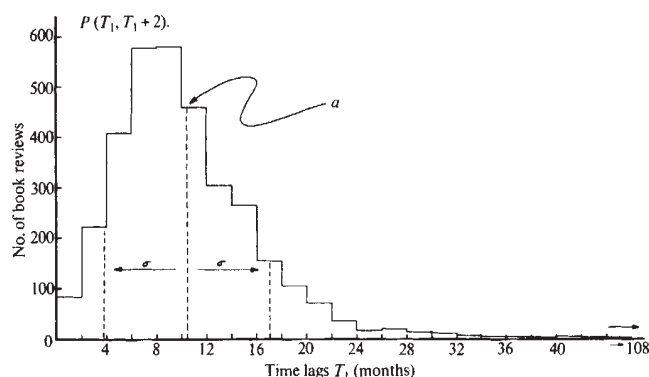


Fig. 1 Histogram of time lag distribution for 3,347 book reviews. a , $T_1 = 10.43$ months (mean time lag for 3,347 book reviews). σ (standard deviation) = 6.63 months.

¹ Bry, I., and Afflerbach, L., *Mental Health Book Review Index*, 8, iv (1963).

² *The Bowker Annual of Library and Book Trade Information*, 70 (Bowker, New York, 1971).

³ Runser, R. E., in *Proc. Tenth Int. Public Library Management*, 43 (University of Wisconsin, 1965).

⁴ Morse, P. M., *Library Effectiveness: A Systems Approach*, 89 (MIT Press, Cambridge, Massachusetts, 1968).

General Books

Bridging the Culture Gap

Science for Non-Scientists: An Examination of Objectives and Constraints in the Presentation of Science to Non-Specialists. By J. S. R. Goodlad. Pp. x+169. (Oxford University: London, January 1973.) £3.25.

SUPPOSE that 40 per cent of our intelligentsia were accustomed to migrate to China at the age of 15. They make themselves quite at home there, and learn to speak and write the language fluently. Ten years later they return, and enjoy thereafter a sort of dual nationality, spending part of each year in Britain, as ordinary family men and women, but going back to China for weeks or months at a time to take their part, also, as citizens of that country. How could they best explain to the stay-at-homes what it is like to live in China amongst the Chinese?

One can imagine some of the methods that might be tried, and can easily guess their consequences. One group might give a general survey of Chinese geography; this would prove very indigestible and uninspiring. Another institution might concentrate on the history of China, which would be interesting for those with a taste for history, but otherwise a little dry and academic. A very advanced and sophisticated teacher might attempt to discuss the current social problems of the Chinese people, but his listeners would find it difficult to relate this to the simple facts of Chinese society. The attempt to distil a few great thoughts and significant concepts out of Chinese philosophy would fail for lack of understanding of Chinese language. A practical course on how to make a Chinese junk would prove amusing, but not very relevant to the main purpose.

In the end, they would probably discover that the only useful thing to do is to invite their friends and relations to visit China themselves for a short time. They might suggest, for example, a short guided tour of one of the great cities, to be delighted and overwhelmed with the splendour of the ancient monuments and the delicacy of the Chinese cuisine. Others might prefer to spend a few weeks living in some quiet village, sharing the daily life of the peasants, helping perhaps to harvest the rice or to build a new farmhouse. The language barrier would still be an obstacle, but with skilled interpreters some modest dialogue might be achieved with the inhabitants: at least the tourists might

come to understand that these creatures were human beings and not goblins or mechanical robots, and that they lived

To make the most of such a programme, it would need to be integrated into the education system at all stages. In the primary school, for example, pictures of Chinese landscapes, little games to dramatize the Chinese way of life, and a few elementary lessons on Chinese ideograms would make children sympathetic to China and its people. Throughout secondary and higher education, this process should continue, so that every educated citizen would be familiar with Chinese ways of thought and action. Unfortunately, there are serious obstacles to this desirable reform. The very sharp segregation of the Sino-logues preparatory to their migration, a peculiar feature of the British educational system, is often anticipated by several years; and those who do not emigrate learn practically nothing about China beyond this stage, and regard it as more fabulous than ancient Cathay.

Indeed, many people argue that the division should be much less definite, and those who go should continue to spend part of each of these formative years in their native land, and that the non-Sinologues should be made to visit China regularly in school parties or even as individual travellers. All these questions are now widely discussed, but nothing is done: schools and universities blame each other for impeding reform, but no common plan of action has been devised.

At the university level, also, schemes for bridging the gap between the two cultures are being tried out here and there, but without great success. Part of the difficulty is that China itself is a very large country: the Cantonese know little of Peking and the Tibetan plateau is very distant from the delta of the Yellow River. Provincial loyalties run

high and the honest simple-minded wanderer must argue his way through many artificial frontiers. The best results have been obtained with a programme of guided visits to selected areas but a whole extra year is regarded as too costly by the powers-that-pay. Perhaps not sufficient emphasis is laid on the exploration of certain common frontier areas: for, if we study *geography* we may observe that China begins, in truth, at Calais; and in the eyes of *psychology* all civilizations are one.

All these points are made thoughtfully, accurately, and with apt citation of many sources, in this excellent book. It is not very long or laborious, or does it pretend to great subtlety nor depth, but it analyses very shrewdly and positively one of the most serious and difficult problems of our present-day culture.

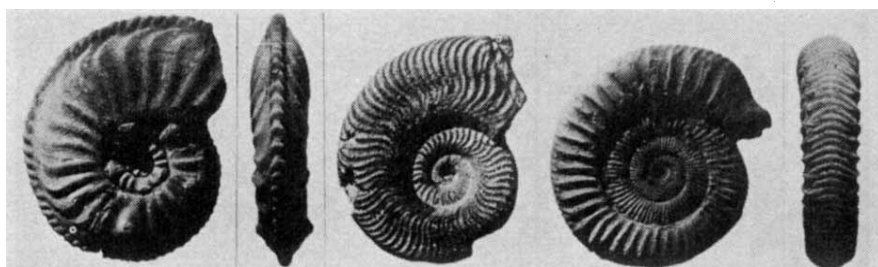
JOHN ZIMAN

Introductory Induction

An Introduction to Confirmation Theory. By Richard Swinburne. Pp. vi+218. (Methuen: London, January 1973.) £3.90.

STUDY of the logic of scientific systems involves problems of inference that are inductive as well as deductive and statistical. After a period of eclipse, the importance of inductive logic is now increasingly recognized, and there is a body of acceptable results and significant problems which is amenable to systematic treatment. Professor Swinburne gives such a treatment in introductory fashion in this book, which will provide a useful textbook for university courses in inductive logic and philosophy of science.

The problem of developing a confirmation theory should be distinguished from two other problems which it does not pretend to discuss. It is not, in the



Typical Pliensbachian ammonites from Württemberg, Germany. An illustration from the *Atlas of Palaeobiogeography*, edited by A. Hallam (Elsevier, Amsterdam, 1973).

traditional sense, a justification of inductive inference. Swinburne makes clear that he is rather drawing out "the criteria implicit in our ordinary judgments" of what it is to be a correct inductive inference. Neither is confirmation theory a theory of statistics, although the most fully developed confirmation theories have been based on relations of probability between evidence and hypotheses and predictions, and this is the approach Swinburne adopts. Calling these "epistemic probabilities", he compares and contrasts them with more familiar applications of the probability calculus in classical, frequency, logical, subjective, and propensity interpretations, and argues for the adoption of the probability axioms in epistemic contexts. This leads him to discuss some traditional problems of probabilistic confirmation theory, including Carnap's conclusion that all universal generalizations have epistemic probability zero, Hempel's suggested criteria of confirmation and his "raven paradoxes", Goodman's "grue paradox", and the question of probabilistic conditions for the acceptance of a hypothesis on given evidence. All these issues are discussed with accuracy and clarity, and often with original and illuminating twists which take them beyond mere recitals of the current literature.

Swinburne is aware that the general principles of epistemic probability he develops have so far been applied only to very simple examples of law-like generalizations, and not to more complex patterns of argument such as are found in scientific theories. Even an example of an analogical argument that he hopes will be "more realistic" (namely "that all specimens of many types of acid have a certain property increases the probability that all specimens of another type of acid will have that property") is not, I think, fully justified by the confirmation principles he has adduced. Goodman's theory of "entrenchment", on which he depends, is not sufficient here, because this inference would be justifiable in scientific practice even before the word "acid" was entrenched in the language. Again the notion of "simplicity" is a very familiar one in scientists' own appraisals of their theories, but Swinburne's chapter devoted to this topic is mainly a discussion of the philosopher's rather parochial "grue" problem, rather than a general analysis of types of simplicity.

The book summarizes excellently the position now reached in the development of confirmation theory, but in so doing it reveals the inadequacy of current ideas to encompass more interesting features of scientific inference, and also it does little to indicate where the points of growth might be.

MARY HESSE

Aero-economics

Air Transport Economics in the Supersonic Era. By Alan H. Stratford. (Second edition, revised and extended.) Pp. xiv+506+8 plates. (Macmillan: London and Basingstoke, 1973. £10.

It is unfortunate that a book about "the supersonic era in air transport" should come out just when that era is in danger of never getting started or at least of being postponed by ten or twenty years. When the first edition of this book was published in 1967 there were still four SST designs in the running—two American, the British and French Concorde and what was to become the Russian TU 144—and it was widely thought that there would be some 500 supersonic airliners operating in the late 1970s, more or less taking over from the B 707s and DC8s just as these had taken over from the old Strato-cruisers and DC7s six years before. Since 1967 the two American SST designs have been abandoned, the estimated selling price of Concorde has escalated from £5 million (with some return on research and development cost) to over £20 million (with no return on a hugely inflated research and development cost), the wide-bodied aircraft have reduced subsonic seat-mile costs by 20 per cent (in real terms), and estimates of sales of SSTs have dwindled from 500 to about 25.

Technology may later learn how to build a supersonic airliner at a more reasonable price (and with more acceptable noise in take-off and landing) so that by 1985 or 1990 it may be possible once more to talk realistically of a supersonic era for air transport as a whole and not merely for a few VIPs in a hurry. In the meantime there are many other developments in air transport that require attention, not so glamorous as supersonic flight perhaps, but much more important both for the airlines and for the great mass of travellers, and Alan Stratford has added a great deal of useful material on these, which, if not factual and conclusive, at least points the way to proper study.

Most important in the immediate present are the sections on the development of jet transportation (chapter 4) and the extremely low fares offered for holiday travel first by the charter operators and soon on scheduled services, which have opened the door to the mass travel market. Using largely depreciated aircraft (or new wide-bodied aircraft) with maximum tourist seating and an 85 per cent load factor, the charter operator can cut 50 per cent off his "direct" or "aircraft" passenger-mile costs, and by wholesaling his seats he can cut his "indirect" or "ground" costs by as much as 80 per cent, thus being able to offer a fare some 65 per cent below the standard scheduled ser-

vice fare. The market for holidays in the sun at these fares is enormous and the total figure of passenger-miles in world air transport (scheduled and non-scheduled) may well continue to expand at its old rate of about 14 per cent per year into the 1980s and beyond. The era that we are entering is not the era of supersonic air transport, but the era of mass air transport.

As before, Alan Stratford is perhaps most useful in those parts of his book on air cargo, where his personal experience is particularly wide. Unfortunately neither the low unit operating costs of the bigger jets nor the mechanization of ground handling, with its pallets, containers and computers, have so far led to the sort of break-through in freight rates that we have achieved in passenger holiday fares. World air freight volume is increasing, but airlines still complain that they lose money on their all-freight services and make a profit only from the supplementary revenue produced by cargo carried in the bellies of passenger aircraft (without this freight revenue, however, the scheduled passenger services would almost universally lose money).

The treatment of R/S/V/Q/TOL (reduced/short/vertical/quiet take-off and landing) is good in theory but perhaps over-optimistic in practice. We still await a V/TOL aircraft quiet enough for city-centre operation and yet large enough and fast enough to have reasonable economics in competition with wide-bodied conventional aircraft on the one hand and the high-speed passenger train on the other. As for R/S/Q/TOL aircraft, it is easy to see their advantages in reducing noise and runway congestion at airports, but difficult to persuade the airlines to buy them when their unit operating costs are significantly higher than those of conventional aircraft. Moreover all these attempts to develop air transportation to city centre or near city centre tend to be frustrated by urban traffic congestion. At many times of day it takes as long for a taxi to go from Park Lane to the Surrey Docks as to Heathrow. The helicopter service from the top of the Pan Am building in New York (and Wall Street), to Kennedy, La Guardia and Newark takes a quarter of the time of the airport bus or taxi, but attracts few passengers and regularly loses money. (It may be that the unbelievable rudeness of New York taxi drivers when asked to do a short down-town trip instead of fifteen miles to an airport has something to do with this.)

Alan Stratford has added substantial sections on cost-benefit studies of airport location, taking into account the extensive work of the Roskill Commission and the many studies of noise nuisance and its assessment that have taken place since the first edition of his

book. These and the bibliographies at the end of each chapter will be found particularly useful to students of air transport even if the input figures and forecasts for the studies and the conclusions of the reports have proved to be unacceptable to the public and parliament.

A. M. LESTER

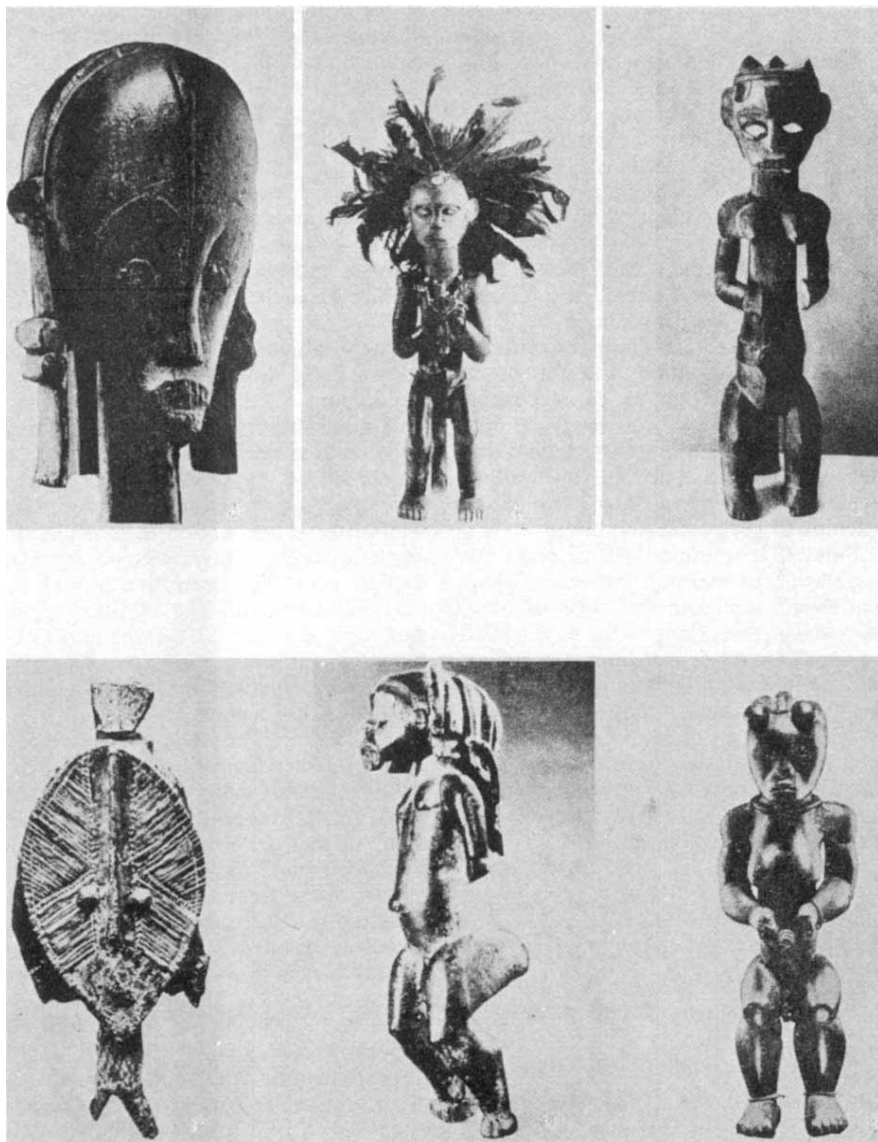
Brain as She is Spoke

The Metaphorical Brain: An Introduction to Cybernetics as Artificial Intelligence and Brain Theory. By Michael A. Arbib. Pp. xii+243. (John Wiley: New York and London, September 1972.) £6.25.

"DISTRIBUTED action-oriented computation in layered somatotopically organized machines . . . for now the reader need simply note that this will prove to be an important catch phrase in our brain theory." I will not say that this quotation from page 5 of Arbib's book sets the whole tone for what is to come, but it does provide a rather disturbing presage. The book is in three parts: an introduction, which includes a certain amount of basic neurophysiology and neuroanatomy, then a section on systems theory and artificial intelligence, and finally a discussion entitled "Brain Theory", much of which has to do with motor systems.

Arbib adopts the portentous declamatory mode, a style affected by a number of cyberneticians. To give another example: "We thus model LTM (long term memory) as *residing in the intervening network between sensory layers and motor layers*. It is represented in the parameters of the network which enable the array of sensory feature activation to be segmented and transformed into a spatially tagged array of OFCs" (the italics are Arbib's and an OFC we have been told previously is an output feature cluster, "the encoding of an object in the action frame, that is . . . a cluster of features that might be appropriate for interaction of the organism with the object"). He balances a fine line between tautology and obscurity, though continually giving a strong impression of the complexity of the mechanisms of the brain. This is not to say that the ideas he is discussing are not important—many of them are, and occasionally he succeeds in being illuminating about them. He is best when describing the action of particular computer programs, for instance, some of those that have been written for the Stanford and MIT robot projects, and also when he describes fairly well understood neurological mechanisms such as the gamma efferent control system of muscles. He surrounds these accounts, however, with a great deal of talk about redundant this, and

Fan Sculpture



Example of Betsi, Ntumu and Mekenya carvings from the Cameroons. In *Statuaire Fan Gabon* (ORSTOM, Paris, 1972), Louis Perrois subjects the primitive art of this region to detailed morphological analysis.

integrated that, freely interleaved with assertions about hierarchically arranged representations of successively more sophisticated behavioural functions. One is reminded very strongly of the writings of Hughlings Jackson. Despite the new vocabulary of cybernetics, much else remains. As in Jackson, here is a set of potentially appealing ideas enveloped in a rather strange and diffuse style. What is more, many of these ideas, having to do with somatotopic organizations and hierarchies of successively more highly evolved neural representations in terms of patterns of action, together with the attempt to pin these down to neurological embodiments, are Arbib's subject matter, and

are exactly the concepts Jackson was wrestling with (though not always successfully) a hundred years ago.

What is required now in brain research is not just ideas juxtaposed in the hope that they will somehow organize themselves into understanding in the mind of the reader. We are in a position to spell out in some detail the formal structure of a few systems capable of supporting certain kinds of mental process. Artificial intelligence and systems theory provide some of the basis for this. It seems possible furthermore that the computer may be an appropriate vehicle for expressing and testing theories of brain mechanisms and mental processes. Brain research

and psychology might in that case be poised for an important conceptual revolution and for fundamental empirical advances. Arbib has selected some of the right general topics, and some of the right substantive material, and many of the ideas that he treats are both interesting and important. However, I have some doubt as to whether the degree of enlightenment he sheds will exceed the amount of mystification. This may seem rather harsh since many of these problems, because of their fragmentary development, are as yet very obscure. Maybe some of the issues which are raised here are too difficult to give any scientific account of at present, even though we can recognize their importance and can see occasional glimmerings of understanding here and there. In this case Arbib may well be justified in flying a few kites in an attempt to get people thinking.

Finally, it is impossible to resist the temptation to mention the quirk which has been adopted in this book of using exclusively the female form of third person pronouns: "it gives the reader all she needs in system theory", and so on. If Michael Arbib means this as a gesture of male solidarity with women's liberation she might have been more appreciated by members of that movement had she written a rather more comprehensible account.

KEITH OATLEY

History of Hydraulics

Hydraulic Power. By Ian McNeil. Pp. ix + 197. (Longman: London, November 1972.) £3.75.

IN Mr Ian McNeil's book *Hydraulic Power*, one of the Industrial Archaeology Series edited by L. T. C. Rolt for Longmans, the author has had two objectives in view. His prime object has been to give a short account of the history of hydraulic power, beginning with the early inventors Pascal and Bramah, and progressing through Armstrong, Tweddell and Ellington to Hele-Shaw, Beacham, Constantinesco, Sinclair, Dowty and Ferguson to reach the present state of knowledge. From 1930 onward hydraulic power experienced a kind of renaissance, particularly through the development of nitrile rubber, a material with physical properties exactly fulfilling the requirements for seals and packings of hydraulic components. His second object is to survey this field for the industrial archaeologist.

The book covers a good deal of ground, dealing with a number of different topics that nowadays would be unlikely to be found together between two covers, even in a book on the history of energy. It is interesting to read, but lacks conciseness. It wanders from one subject to another. The chapter head-

ings have no standard of parity, emphasizing this desultory treatment. Some of the illustrations are not very clear—for example, Armstrong's rotary hydraulic machine of 1838—whereas a few, such as that of Elisha Otis's safety lift, are quite fascinating.

A number of interesting schemes that have since been abandoned are considered—for example, Constantinesco's wave transmission. The most noteworthy sections include chapter 7, dealing with hydraulic mains systems, municipal and metropolitan, and drawing attention to the fact that a great many of these networks are still in existence and in daily use. Chapter 8, "Cranes, Hoists and Lifts", reminds us of the lead taken over by the USA in many large scale projects from 1866 onwards, which led to its primacy later in the mining and construction industries. Also interesting is the section on fluid coupling and torque converter (chapter 11), though here the description would be much clearer with the addition of a diagram. Chapter 12, "Twentieth Century Renaissance", pinpoints the part that many small improvements have played in this regard.

In promoting his second objective, the author has interpolated the views of the industrial archaeologist throughout the book, and has carefully described a great many of the earlier hydraulic machines, with their auxiliary apparatus, pump-houses, etc., many of which are still working, or have been preserved by museums or other agencies. He gives careful directions to sites where they can be seen, and a short gazetteer completes the book. This information is important and valuable, both as a record of the machines and as a welcome source of reference for the industrial archaeologist.

AUBREY BURSTALL

Moseley and the Elements

Moseley and the Numbering of the Elements. By Bernard Jaffe. Pp. x + 263. (Heinemann: London, November 1972.) £1.50.

It is always interesting to ask what opinion the biographee would form of his biography. In this case one can, I think, be quite certain of the answer: Moseley would have held this work in contempt. Jaffe writes in his preface of "writing a full biography" and appends a "Complete List of Research Papers by Moseley"; he implies that he has spent ten years gathering material and thanks nearly thirty persons, including a score of Moseley's acquaintances, for aiding his researches; he has found in the archives and quotes at length (though not always in full) the known letters from Moseley to Rutherford, C. G. Darwin, and N. Bohr. But this material, which apart from the letters proves to be negligible, is then folded

into a pabulum of romanticized science digested to gibberish for consumption by pre-adolescents. There are sections—for example that on the discovery of the diffraction of X-rays by crystals ("chapter 4. Moseley is Stirred by the Crystal Gazers")—which may be taken as veritable paradigms for wanton carelessness of factual accuracy and traduction of physical principles. This slough of confusion (as to genre and audience as well) was originally published as one of the paperbacks in the "Science Study Series" intended as supplementary readings for a distinguished American secondary school physics course. It here appears in its British edition between hard covers at twice the American price but without the collection of plates in the original.

In his preface Jaffe rues his failure to turn up the very extensive collection of letters which Moseley wrote to his mother and sister, recognizing perhaps that only they would give the record of Moseley's experiences, activities, and personality upon which an adequate biography could be based. But these letters have in fact come to light; together with Moseley's scientific correspondence they have now been edited and annotated by John L. Heilbron: *H. G. J. Moseley: The Life and Letters of an English Physicist, 1887–1914* (University of California Press, Berkeley, in the press). It is to Heilbron's scholarly study that anyone with a serious interest in Moseley and the numbering of the elements will have to turn.

PAUL FORMAN

Unifying Principles

Statistical Mechanics: New Concepts, New Problems, New Applications. Edited by S. A. Rice, K. F. Freed, and J. C. Light. (Proceedings of the Sixth IUPAP Conference on Statistical Mechanics.) Pp. viii + 423. (University of Chicago: London and Chicago, 1972.) £7.20.

THE conference that brings active workers in related disciplines together is rightly becoming fashionable. We have here twenty-one papers ranging over statistical mechanics and the related fields of hydrodynamics and cybernetics given in March/April 1971. A glance at the list of distinguished contributors shows that it must have been very enjoyable indeed to listen to their interactions, some of the flavour of which has been preserved by recording the discussions after each paper. For example, it emerged that, rather disgracefully, very little is known in general about the limiting behaviour of equilibrium properties near absolute zero temperature, which is a little surprising when one considers all the work that has gone into studies of ground and low-lying states of particular systems.

Also that there is often very real doubt on how to choose "good" variables in particular problems.

The organizers, rightly in my view, have a horror of parallel sessions. For time-table purposes, there were five successive sections: "Fundamental Principles", "Developments in Biology", "Generalized Hydrodynamics" (including turbulence), "Phase Transitions" and "Liquids", the papers being printed in the order in which they were actually given. (In fact, there was a great deal of overlap between the sections.)

The papers are all of a high standard and many of them will be valuable for a long time to come as review articles, but it would be quite wrong to conclude that anyone was merely "doing his own thing"—it is absolutely clear that everyone had other fields very much in mind. For example, there are very interesting discussions of the concept of a limit cycle. This is fundamental in mechanics, in turbulence and in neural networks, yet ergodic theory has gone to great pains to try to show that no such thing occurs

(or that it can be neglected as "exceptional" if it does) in the "ordinary" systems studied in statistical mechanics. Another very interesting general concept is that of static or dynamic scaling. The consensus here is that there is "a great deal in it", even though very little can be rigorously proved at present.

The book has been produced by photographing typescript on to very good paper: the result is easy and pleasant to read. I certainly recommend it to any serious worker in any of these fields. H. N. V. TEMPERLEY

Physical Sciences

J. A. Wheeler

Magic Without Magic: John Archibald Wheeler. Edited by John R. Klauder. Pp. xiii+491. (W. H. Freeman: San Francisco and Reading, January 1973.) £9.20.

THIS volume contains the *Festschrift* collected to honour J. A. Wheeler on his sixtieth birthday. In it a galaxy of physicists has contributed essays on topics to which Wheeler has made important contributions. Many photographs are inserted at various places in the book and the volume starts with an introduction in which a number of people give their impression of Wheeler: one of the executives of DuPont describes Wheeler's impact on the work at DuPont for the Manhattan Project to design a plutonium manufacturing plant; Teller describes Wheeler's contribution to the hydrogen bomb project, and Spitzer his work on Project Matterhorn; the chairman of the Princeton Physics Department, one of the Trustees of the Batelle Memorial Institute, and the Director of Institute Research in Physical Sciences of the Batelle Memorial Institute recall Wheeler's contributions to easier communication between scientists and politicians; finally, the President of Stony Brook tells about some of Wheeler's work in the early days of particle physics. This introduction gives an excellent impression of Wheeler, the physicist and man; it is complemented by the last essay, "The Anatomy of Collaboration", by E. F. Taylor who wrote with Wheeler the introductory relativity textbook *Spacetime Physics*.

The essays between the introduction and Taylor's essay are original scientific articles stemming in one way or another from the authors' association with Wheeler (to quote from the preface). The first two essays deal with the deformation of nuclei—studied in

the classic Bohr-Wheeler and Hill-Wheeler papers—and the related problem of the existence of superheavy nuclei. The next two essays consider mesic atoms, a topic broached by Wheeler in 1949. Wheeler and Feynman considered retarded and advanced effects in 1949; this subject is taken up by Sygne in an essay on the electrodynamic double helix. Then follows an essay on retarded long-range interaction potentials between molecules—a topic treated by Wheeler in 1941.

It is not generally known that although Heisenberg coined the expression S-matrix in 1943, the concept of a collision matrix, that is, a way of characterizing a collision process by specifying the ultimate state of the system was introduced by Wheeler in 1937. Two essays, by Goldrich and Wigner and by Regge, consider the properties of the S-matrix. As Wheeler himself has shown the power of what Maxwell called the "cross-fertilization of the sciences", it is fitting to find in this *Festschrift* an essay describing experiments to test various aspects of relativity using, respectively, gyroscopic precession, nuclear electric dipoles, and superconducting flux quantization.

In recent years Wheeler's main interest has lain in the theory of relativity, covering such aspects as gravitational collapse, Mach's principle, super-space, geometrodynamics, and the quantization of gravitation. It is therefore not surprising to find that more than half of the essays are dealing with these and other aspects of the theory of relativity. Among the authors we find the Russian experts Zel'dovich, Khalatnikov, and Pokrovski, as well as Western specialists such as Penrose and Dicke, to name but two.

Altogether this volume provides the reader with a veritable feast, well worthy of the man in whose honour it was written.

D. TER HAAR

Solid State Chemistry

Preparative Methods in Solid State Chemistry. Edited by Paul Hagenmuller. Pp. xvii+602. (Academic: New York and London, December 1972.) \$36.

SOLID state chemistry is not too well served with books. Because its subject matter overlaps with other disciplines, and the motive for much recent work has had a physical or technological basis, the relevant experimental methods are dispersed through a wide range of journals. The idea behind Professor Hagenmuller's book is therefore welcome.

What is involved is the study of phase equilibria, solid state reactions and the synthesis of pure materials, usually by high temperature methods. The combination of high temperatures with very high pressures has opened up much new chemistry, and the book starts well, with chapters on high pressure techniques (by Rooymans) and high pressure synthesis (from MIT Lincoln Laboratory, where it has become the standard procedure). There is also an uninformative chapter on Russian work using transient shock wave methods. Thereafter, the balance of the book is somewhat impaired by over-emphasis on crystal growing. Important as this is for technology, it is only one aspect of preparative solid state chemistry. Methods are reviewed, in relation to high-melting oxides, by Anthony and Collongues. It is to be regretted that they give only scanty, incidental attention to the techniques of high temperature experimentation, for Collongues and other French workers have done much work on reactions and phase studies above 2,000 K. A chapter on such methods would have enhanced the value of the book. The same deficiency on the side of high temperature technique detracts from the otherwise useful chapter, by Windisch

and Nowotny, on carbide chemistry. With other chapters on chemical vapour transport, synthesis by electrolysis of fused salts and fluorine chemistry, about two thirds of the book can be welcomed as a useful source of information to those working in or entering the field.

The remainder of the book is probably unduly specialized: preparation of single crystals of III/V semiconductors and of cadmium sulphide, preparation of ferrites, oriented eutectic crystallization and so on, together with a chapter on elementary boron which is remarkable for its complete failure to mention the curious metallic borides, of very high boron content, which so long masqueraded as the element itself. These topics are important for technology, but they overweight one facet of the subject and some of them have been repeatedly discussed in other books. One saving point is that the writers have used the opportunity to deal with the principles underlying particular problems.

The book as a whole emphasizes principles and objectives, as well as methods. The editor has managed to get his collaborators to cover the ground without much overlap or repetition—even though one diagram, of a hydrothermal bomb, does appear in two different chapters. The book is well produced—as indeed it should be, at the price.

J. S. ANDERSON

Atmosphere

Handbuch der Physik/Encyclopedia of Physics. Gruppe 10: Geophysik, Band 49. Teil 4. Geophysik 3/Geophysics 3. Teil 4. Chief editor, S. Flugge. Edited by K. Rawer. Pp. vi+579. (Springer: Berlin and New York, 1972.) 158.40 DM; \$50.30.

THIS is the fourth volume of a handsomely produced series covering several aspects of solar-terrestrial phenomena. It deals with ionospheric and magnetospheric problems and, as the editor K. Rawer points out in his introductory remarks, there is some overlap not only between the first two contributions but also with some articles published in earlier volumes. This was to some extent unavoidable. Again, there has been some delay, as much as 5 years, in the publication of these articles; in a rapidly advancing subject such as this, this may be said to be a serious drawback. It by no means detracts, however, from the considerable and valuable accounts given of ionospheric and magnetospheric phenomena.

I found the first article, by Dr H. Poeverlein, on the Earth's magnetosphere particularly rewarding. There is a good and novel account of magnetodynamic theory. A section on fast

particles trapped in the geomagnetic field naturally includes some account of the radiation belts around the Earth, and discusses such problems as the source and loss of particles, their collisions and diffusion and the acceleration of trapped particles in which Alfvén's mechanism, proposed originally in a theory of magnetic storms in 1939, is cited as an example. Convective motions in the magnetosphere, the solar wind and consequential terrestrial phenomena associated with the interaction of the wind with the geomagnetic field are included—for completeness, I think—and aurorae are also discussed at some length. There is a short section, contributed by N. Fukushima, dealing with more recent work such as the polar cusps, some recent theoretical work on the geomagnetic tail and the strong coupling between magnetosphere and ionospheric currents.

Dr Hess's article on the Earth's radiation belts naturally begins with charged-particle motions in a magnetic field. The inner and outer radiation belts are discussed at considerable length and there is a valuable account of the artificial radiation belts created by exploding nuclear devices high up in the atmosphere (including the Argus—the planning for which was well under way before the discovery of the van Allen natural radiation belt—Starfish and the USSR experiments).

The general characteristics of the outer radiation belt protons, α -particles and electrons are fully discussed, as are also their time variations, dependence on solar activity—including magnetic storm effects—aurorae and their associated X-ray fluxes. The article ends with a brief description of the radiation belts of other planets.

Dr Selzer's long article on rapid geomagnetic variations gives detailed definitions and discusses the various techniques of observations. Dr Selzer also considers at length the classification and morphology of the variations and, since these pulsations are usually ascribed to magnetospheric phenomena in which wave propagation plays an important part, there is naturally a theoretical section on the interpretation of the pulsations. The article includes an atlas of 69 plates showing examples of different types of observed variations including sudden commencements of storms (SSC) and so-called "pearls" or pc-1, pi-1, etc.

Finally we have a masterly article on waves and resonances in magneto-active plasmas, written jointly by Drs V. L. Ginzburg and A. A. Rukhadze. This highly mathematical and exhaustive review of the subject deals with foundations of plasma theory, particle collisions in plasmas, waves in plasmas—including magneto-active plasmas—the stability problem and oscillations

and waves in inhomogeneous plasmas. The approach is sometimes novel, as in dealing with the collision integral in a completely ionized gas, where the authors apply the methods of quantum mechanics rather than the classical method of collisions. In writing this article the authors have borne in mind the ultimate applications of the theory to the ionospheric and magnetospheric plasmas.

The present editors of the *Handbuch* are to be congratulated on the excellence of the volumes on geophysics which they have produced. Their inception, I believe, was due in large measure to the work and enthusiasm of the late Julius Bartels.

V. C. A. FERRARO

Noble Metals

Recent Advances in the Analytical Chemistry of the Noble Metals. By F. E. Beamish and J. C. Van Loon. Pp. xvi+511. (Pergamon: Oxford and New York, March 1972.) £14.

THIS book provides information on new techniques for the isolation and determination of the noble metals. It complements and updates the earlier excellent monograph in which Professor Beamish described the classical and some modern methods. It would be difficult to identify any significant publication on the analysis of noble metals which has not been referred to and received some critical examination in these monographs.

There are eight chapters covering methods of separation, atomic absorption, neutron activation, spectrochemical, X-ray fluorescence, electrochemical, spectrophotometric, gravimetric and titrimetric procedures. Each chapter starts with an introduction which considers the techniques available and points out advantages and disadvantages. Frequently, areas where more research would be profitable, for example direct weighing precipitates for iridium, rhodium, platinum and ruthenium are identified. The authors also point out where much more research effort has been expended than is justified, for example spectrophotometric methods for palladium. For each element the alternative procedures are discussed and some suitable procedures selected for which practical details are given fully. Then follows a comprehensive review of published methods in which the authors' critical comments and identification of significant points are of considerable value.

The section on methods of separation includes ion exchange, precipitation, chromatography, solvent extraction and fire assay. The recently introduced technique of collection of noble metals in nickel sulphide could be usefully included in later editions.

The application of atomic absorption to noble metal analysis is satisfactorily covered and the twelve methods described in detail include applications to silver assay beads, geological materials and concentrates.

The information on neutron activation techniques is comprehensive and illustrates the wide applicability of these high sensitivity (sub p.p.b.) methods for any analyst having access to a suitable reactor.

The chapter on spectrochemical methods includes applications to rocks, ores, base metals and other noble metals. Particular attention is paid to pre-concentration. Spark source mass spectroscopy receives only a brief reference. Although X-ray fluorescence analysis plays an important part in the control of refining processes, information published on its applications is not extensive and this is reflected in a short chapter (14 pages) on this technique in which only three rather specialized methods are detailed.

The spectrophotometric section with 120 pages and forty-two practical procedures is extensive and there are comprehensive literature references. The summary of information in the tables is particularly noteworthy. Electro-metric methods include polarographic, coulometric, amperometric, potentiometric and gravimetric procedures. The chapters on gravimetric and titrimetric methods record few improvements over those reported in the previous monograph.

This book and Professor Beamish's previous monograph together constitute the most comprehensive collection of information available concerning the analysis of noble metals.

W. WESTWOOD

Indicators Unlimited

Indicators. Edited by Edmund Bishop. Pp. x+746. (Pergamon: Oxford and New York, December 1972.) £12.

In view of the well known facts about elephants and their gestation period, it comes as no surprise to learn that some of the contributors to this book found their material needed updating by the time the last sections had come in. A further parallel with the elephant is that very little indeed seems to have been forgotten. One wonders, however, whether the editor should not rather have styled himself a literary midwife, since by his own admission the editing was minimal, which is a pity because a little assistance here and there would have clarified certain parts for the reader. That the contributions by the Hungarian authors read so well is a tribute to the linguistic prowess of the writers.

Most practising chemists will undoubtedly use the book as a source of

information about the indicators themselves, and skip the sections on theory. Those who probe further, however, may find a few questions arising. For example, on page 19 we have ion combination reactions giving rise to sparingly soluble molecules, which seems rather contradictory in terms. The word "precipitometric" on the next page seems rather undesirable, and is followed by "principal reagent indication" which turns out to mean that the titrant serves as its own indicator in some way or other. Only two examples are quoted, and Aberdonians would like to see Clark's soap solution mentioned. One class of self-indication that is omitted is the disappearance of a precipitate as a result of complexation (as in the cyanide titration of nickel). It seems curious that self-indicator systems are treated so cavalierly (one paragraph and half a table) when the theory of redox systems seems to be dealt with three times over.

The discussion of one-colour indicators appears to overlook the fact that if the transition is from the colourless to the coloured form, then the first appearance of perceptible colour is taken as indicating the end-point, and the point at which this appears is dependent on the concentration of the indicator. Discussion of transition intervals for complete development of the colour seems slightly irrelevant. On page 54 there is a curious omission of the titrant concentration in the second line. The discussion of Job plots (page 59) omits to mention that they are not satisfactory for measuring very low values of formation constants and that the extrapolations are affected at least as much by errors in measuring absorbance as by errors in mixing the solutions. Indicators for determining the Hammett function are dismissed in a few lines, and the work on complementary tristimulus colorimetry in not much more.

On page 22 we have a "term under the log" (something nasty in the woodshed?), the third sentence on page 24 has a misplaced comma which acts as a full stop for the reader, and lower on the same page an indicator is busy converting logarithms to base 10. Page 31 says that curves were drawn on a computer, which is all that some people think computers are fit for.

Closer attention to proofreading might have avoided the mis-spelling of the name of one of the general editors, the address of the first contributor, and Volhard, and the introduction of a Scotch error on page 54. Sometimes there are different symbols for the same quantity, on the same page, and different spellings for the same compound (for example lucigenin(e)). The sign $\rightleftharpoons$ is used in one section instead of $\rightleftharpoons$ to indicate equilibrium.

These comments refer to minor blemishes on what, after all, is the most comprehensive work of its kind and one that will be pre-eminent in its field for a long time to come. They are made here lest the reader, overwhelmed by the massive compilation of information, imagines that a bishop must be as infallible as a pope.

R. A. CHALMERS

Wave Functions

Molecular Wave Functions and Properties: Tabulated from SCF Calculations in a Gaussian Basis Set. By Lawrence C. Snyder and Harold Basch. Pp. vii + 431. (Wiley: New York and London, January 1973.) £6.65.

THIS book is a tabulation of the ground-state wave functions calculated by the two authors for some fifty-six different molecules, all built up from hydrogen and first-row atoms of the periodic table. The first forty-two pages are introductory and serve to explain the nature of the calculations set out in the remaining 400 pages of text. These calculations cover molecules ranging from the hypothetical borane BH_3 with eight electrons to the forty-eight electron molecule C_2F_4 . Only six of them are diatomic molecules—a very proper situation, since there would be little to say in favour of duplicating the existing tables of McLean and Yoshimine dealing with small linear molecules.

The simplest way to describe the contents of the book is to say, first, what numerical magnitudes are tabulated and, second, how they were obtained. Each molecule is represented by a table of up to eight pages. These pages begin with the molecular geometry (group symmetry and Cartesian coordinates of the nuclei, normally taken to be at the positions found by experiment); next comes an account of the various symmetries of the MOs, and a set of total energies (for example, total potential energy, total kinetic energy, total electron-electron energy, and so on). This leads to tabulated electronic moments of the final charge distribution taken up to fourth order. Then come a variety of other expectation values, such as powers of r , measured from individual nuclei) and the various quadrupole coupling terms. Mulliken charges and overlap populations lead to a summary of the coefficients that come in the LCAO forms of the MOs, and a final table of overlaps.

A strong point of the collection is that one set of atomic functions has been used throughout so that comparisons between different molecules is both straightforward and justifiable. Almost all the tables were printed photographically from a computer output so that few, if any, errors are likely to remain.

The calculations all lie within the framework of the single-determinant MO-method. As is usual nowadays each MO is expressed in LCAO form, and the expansion coefficients are found by the standard Roothaan technique. The component atomic orbitals have radial factors which are the sum of two parts, analogous to the double- ζ form using Slater orbitals. Here each part is a standard combination (or contraction) of two, or four, Gaussian functions, obtained as "best-atom" functions and incorporated into the POLYATOM program which was used. This leads to a total molecular energy about 0.05 a.u. per atom above the Hartree-Fock value. For many purposes this accuracy is sufficient: to improve on it would require configuration interaction and the numerical values of the inaccessible two-electron molecular integrals.

It is difficult to see just how important tables of this kind will become. Larger research centres will frequently have their own means for direct calculation; moreover there are many thousands of molecules which might claim a right to inclusion in a compendium of this sort and, lacking any possible incorporation of configuration interaction, it can say relatively little about excited molecular states. But this volume will prove useful and—in an age of economy—it is a good deal cheaper to buy it than use the program on which the results are based.

C. A. COULSON

Dynamics

Dynamic Behaviour of Processes. By John C. Friedly. Pp. xv + 590. (Prentice Hall: Englewood Cliffs, 1972.)

DYNAMICS is an essential part of most engineering disciplines, but the corresponding component in chemical engineering, process dynamics, has never quite been regarded as an entity of its own. This is almost certainly due to the dominance of steady-state operation in most physical and chemical processes. Accordingly, the study of process dynamics has been linked to that of process control, with far greater emphasis, in general, on the latter. In this book, Friedly has attempted to show that process dynamics has a place of its own in areas other than process control. I believe he has presented his case extremely well.

The greatest contribution of the book is a consistent, uniform, treatment of a wide range of dynamic problems with examples taken from a cross-section of recent chemical engineering literature. In each example the previously derived matrix equations are fully expanded, thereby illustrating the application of the relevant theory clearly and without

ambiguity. While the development of the theory is complete and self-contained, here again the emphasis is placed primarily on applications rather than on mathematical rigour.

After several introductory chapters on typical models and mathematical techniques, the book is divided into two (roughly) parallel sections on lumped and distributed parameter systems, dealing with linear and non-linear examples of both. The structure of partial differential equations is discussed in considerable detail, including illustrations of how the characteristics of the equations affect the dynamic behaviour of the corresponding systems.

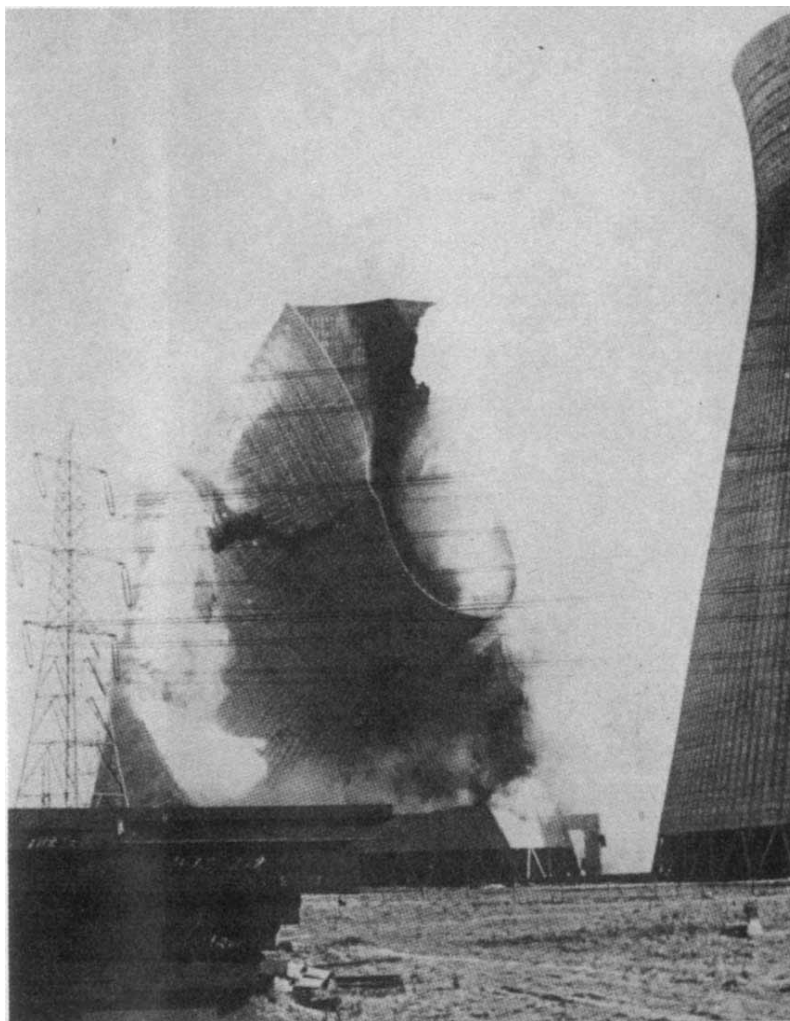
Several chapters are devoted to state space representation of dynamic problems. On these occasions the author has taken great care to point out the

relationship between modern state space representation and the conventional input-output (Laplace transform) approach. This should prove extremely valuable to students with a prior knowledge of elementary classical control theory.

The one drawback I can see with the author's approach is that, necessarily, several applications in the area of process control are not carried to their ultimate ends. This is especially true with optimal control, the subject which stimulated the growth of much of the theory discussed in the book. By attempting to stand process dynamics on its own, it was perhaps inevitable that the crutch of control theory be cast off, but a more complete picture might have been obtained otherwise.

L. KERSHENBAUM

Ferrybridge Power Station



The day the cooling towers blew down at Ferrybridge C power station, Yorkshire (November 1, 1965). An illustration from *Britain's Weather, Its Workings, Lore and Forecasting* by David Bowen (David & Charles, £3.25), a non-specialist account which includes a description of the work of the Meteorological Office, and some hints on how to do your own forecasting if the Met. Office leaves you dissatisfied.

Chemical Kinetics

Chemical Kinetics. Edited by J. C. Polanyi. Consulting Editor A. D. Buckingham. Pp. 322. (MTP International Review of Science. Physical Chemistry. Series one, Volume 9.) Pp. 322. (Butterworth: London; University Park: Baltimore, Maryland, 1972.) £10.00; \$24.50.

TWENTY-TWO years ago, volume 1 of *Annual Review of Physical Chemistry* was inaugurated with an ambitious survey, including eighteen sections, one of which, *Chemical Kinetics*, gave a reasonable coverage of that then relatively young field. It is a reflexion of our time that Butterworth has now announced a new series to review biennially all of chemistry in 33 volumes and three indices. Volume 9, *Kinetics* is one of thirteen volumes of the series devoted to physical and analytical chemistry. It contains ten chapters by very well-known research workers under the categories of reactions: unimolecular, by D. W. Setser, including for the first time that of ions; bimolecular, by I. M. Campbell and D. L. Baulch, mostly of atoms and small radicals; free radicals, by E. Whittle; hot atoms, by F. S. Rowland; chemiluminescence, by T. Carrington and J. C. Polanyi; molecular beams, by J. L. Kinsey; ion-molecules, by J. Dubrin and M. J. Henchman; solvated electrons, by F. Dainton; energy transfer, by J. I. Steinfeld; and relaxation techniques, by J. E. Crooks.

The material covered extends, in general, from 1967 to 1971 though many of the authors have chosen to avoid overlap with recent reviews, and so cover only the period 1970–1971. The second set of thirty-three volumes will cover the period 1972–1973 and is scheduled for 1974. The style is that of the typical review and does not lend itself to easy reading, except by specialists. The reviewers in general, however, write well and with authority so that the volume is excellent for conveying to the “cultured” scientist current progress in each field and some flavour of the directions and interests of kineticists today. Its greatest usefulness, however, will be to the research worker and specialist for whom it will represent an appreciable saving in time. In spite of its length, it suffers from the fault of most reviews of being descriptive rather than critical, so that apparent or hidden contradictions are not resolved. This is possibly the inevitable state of any active research field. There are a relatively few, but glaring typographical errors.

In these days of shrinking library budgets and expanding scientific literature (both original and review), the author refrains from judgments on the transient or ultimate utility of so imposing and expensive a project. At

about £10 (\$24.00) per volume, it will represent a forbidding investment in money and space to both libraries and research workers. Perhaps this only underlines one of the most painful and critical weaknesses of science today—our archaic communications system.

S. W. BENSON

Statistics of Spectra

Gamma-ray Spectroscopy, with Particular Reference to Detector and Computer Evaluation Techniques. By P. Quittner. Pp. 111. (Adam Hilger: London, 1972.) £4.

THERE have been two revolutions in the field of gamma-ray spectroscopy since 1946. The first came in the late forties with the advent of the scintillation counter. This provided the first efficient gamma-ray detector as well as a spectrometer of rather poor pulse height resolution.

The emergence of the Ge(Li) semiconductor detector in the early sixties produced the second revolution. With this detector, the high counting efficiency attained with the scintillation counter could now be complemented with a superb pulse height resolution which, in many cases, can surpass the best resolutions obtainable with magnetic and crystal spectrometers.

This little book is not primarily concerned with the physics of these counters, but rather with the many elaborate statistical techniques that have been devised as aids in the interpretation of their spectra. As such, it has been written very much for the specialist and does not make light reading.

Its main part surveys techniques of spectrum smoothing, detector response function, peak location, peak area determination, weighted least squares resolution and spectrum stripping. Another chapter is devoted to miscellaneous applications such as decay curve analysis programs, detection limits and on-line applications.

The value of these surveys lies not so much in their aiding the implementation of the techniques, but rather in a discussion of their scope, supplemented with a lengthy list of references. One shortcoming is the fact that, although these techniques rely implicitly on computers for their execution, very little mention is made of the many programs that are now accessible to all workers; but maybe this is too much to expect from a work of this size.

This book can be expected to play an important role in the future for workers in the fields of nuclear and particle physics, activation analysis, X-rays, and more generally workers in many other fields involving statistical observations.

D. K. BUTT

Earth Science

Geophysical Surveys. Edited by Wm. Markowitz. (*An International Journal of Geophysics.*) Vol. 1. No. 1. Pp. 119. (D. Reidel: Dordrecht, September 1972.) \$25.29; Dfl. 170 (libraries and institutions). \$22.75; Dfl. 70 (individuals).

THE appearance of a new earth science journal is not nowadays such a rare event. Over the past few years a number of publications, each catering for a limited range of interest, have appeared, the great majority of them having their birth in publishing houses and not the learned societies.

The title “Geophysical Surveys” means “exploration” to most earth scientists, and “Geophysical Reviews” might better describe the contents of the first issue and indeed the aim of the journal as a whole. The object of the publication is to provide a contemporary synthesis of progress in various branches of “geophysical research”. Each article is preceded by a full explanatory introduction to give background and aid in understanding the significance of the work.

This first issue contains papers on earthquake prediction, the Rhine Rift Valley, the hydraulics of the stream flow, tidal flow and the Earth's core. Papers are by invitation only and original research papers will not be included. The papers are indeed informative and there is certainly a crying need for papers of this kind—and not just in this branch of science—which collates a lot of information together in one place. It is almost impossible nowadays to keep abreast of all journals in earth science, in a particular field, and the thought of reading one journal, which updates progress from time to time, will doubtless appeal to many.

Even so, for this privilege institutions are asked to pay about £23 a year, whilst individuals receive the quarterly issues at about £9.50 which at nearly £2.40 an issue borders on the expensive. Would-be readers will have to decide if the convenience of these reviews justifies the price.

J. R. V. BROOKS

Pulp and Polymerization

Chemical Modification of Papermaking Fibres. By Kyle Ward, jun. Pp. viii + 246. (Marcel Dekker: New York, December 1972.) \$18.50.

THE orthodox maker and user of paper tends to treat his fibre as an inert material and to forget that cellulose is both a polymer and an alcohol and therefore capable of entering into many interesting and possibly useful chemical combinations. The chemist who wishes to study more thoroughly the reactions

which cellulose may undergo has to wade through masses of literature primarily concerned with either textiles or polymers. This may be tedious and can be annoying. One can find large books on these subjects in which "paper" appears rarely in the index and one can even find tabulated data on cellulose fibres in which wood pulp, in terms of tonnage the most important of all, is not mentioned.

The book by Emeritus Professor Kyle Ward fills a necessary gap. It will do much to encourage the reader primarily interested in the chemical properties of pulp and paper fibres. The reactions of pulp and paper fibres are grouped in chapters on esterification, etherification, oxidation, cross linking and graft polymerization. Nearly all the references are directly concerned with pulp and paper and in each chapter the author discusses possible uses of the reactions described. Many of the references (there are over 800) are to patents, frequently taken out by chemical manufacturers.

Cross linking has been used for over thirty years in making "wet strength" papers but the cross linking references are all in one chapter and this is helpful to the theorist. Alkyl ketene dimers have now been sold for sizing for eighteen years but we find them in the same chapter, on esterification, as the older processes for partial acetylation of paper for dielectrics. The grouping of reactions according to their chemistry is refreshing—in most books on paper making they would be grouped according to their part in the process.

These are important differences between pulps due to the effects of side chains. Paper made from pulp with hydrophobe side chains is weak, unless made from a non-aqueous solvent, but paper already made can be chemically modified without loss of strength.

We should be grateful that Professor Ward has found the time and energy to write this book. He has made good use of the unrivalled library facilities at the Institute of Paper Chemistry, Appleton, Wisconsin, for the benefit of all of us.

F. LYTH HUDSON

Physical Geography

Environmental Geomorphology and Landscape Conservation. Edited by D. R. Coates. Volume I: Prior to 1900. Pp. x+485. (Dowden, Hutchinson and Ross: Stroudsburg, Pennsylvania, January 1973.) £9.75.

THIS is the first of three volumes concerned with man's use of his physical environment, forming part of a "Benchmark Series" of geological texts. The aim of the series is to ease the availability of many important papers by reproducing them in a facsimile fashion along with sectional editorial comments.

In this particular volume, variation in the quality of reproduction is a reflexion of the original material but the photographs have not generally reproduced well. The book would be far easier to read had all the papers and diagrams been re-set and re-drawn. Professor Coates has collected together thirty-three diverse publications on the theme of man's exploitation of his environment before 1900.

The text is subdivided into five sections: the first illustrates the importance of water to man. The next contains articles by Barnes and Semple—both showing the influence of terrain on man. It is a pity that these publications on geographical philosophy represent solely the deterministic outlook. The third section contains a variety of descriptions of the United States from the early eighteenth century to the rather interesting First Annual Report of the United States Geological Survey in 1880. The effect of man on the physical environment is well illustrated in the fourth section, with articles on such topics as the reasons for the decline of the Roman, Mayan and ancient Mesopotamian civilizations. It is most fitting that this section is concluded by a paper by G. K. Gilbert who, in examining the effect of sediment production from hydraulic mining, raised many environmental themes of marked relevance today. The final section on landscape conservation includes two papers on ancient agriculture in the Negev desert which have relevance to present day agriculture in that area. Other papers in this section are representative of early writings on the need for conservation techniques in agriculture. A short extract of Sir John Sinclair's writings is included; it tends to be forgotten in Britain today that the need for soil conservation measures has been stressed since the early days of the agricultural revolution.

The only general criticism which can be made of the book is its title: many of the papers cannot be termed truly geomorphological; and geomorphology must, by definition, be environmental. Instead the book is a collection of essays on physical geography and some other title ought to have been devised. The title, however, is of little importance compared with the contents. The two principal contributions of this book are to present in one volume many diverse publications and, more importantly, to add a great deal of perspective to present day environmental debates. This is achieved by showing that man from very early times has greatly modified his environment, often consciously, and hence some of the consequences of present day exploitation ought to be assessed by reference to past events.

DONALD A. DAVIDSON

Microwave Ultrasonics

Microwave Ultrasonics in Solid State Physics. By J. W. W. Tucker and V. W. Rampton. Pp. x+418. (North-Holland: Amsterdam, 1972.) Dfl. 130; \$40.75.

SOME justification is needed for a volume on ultrasonics that deals only with frequencies above the apparently arbitrary limit of 1 GHz. The explanation is that 1 GHz was the maximum frequency that could be achieved by the traditional method of ultrasonic generation using a resonant piezoelectric transducer. Recently, techniques have become available for obtaining frequencies much higher than this figure, as high as 114 GHz in one experiment, but more typically in the range 3 to 10 GHz. The authors of the present book state that it has been their intention to concentrate on the areas of solid state physics that have become ultrasonically accessible only as a result of these advances. In this aim they have generally been successful, although their choice of material does sometimes appear a little arbitrary and incomplete.

The book begins with two short chapters embodying a brief historical development of the field of microwave ultrasonics and a short theoretical survey of elastic wave propagation in solids. An experimental chapter describes in detail the techniques that are used for generating and detecting ultrasound at microwave frequencies. Particular emphasis is given to non-resonant piezoelectric, magnetostrictive, and thin film transducers. Satisfying experimental detail is provided on such aspects as the design of microwave cavities, the preparation of specimens, and the evaporation of piezoelectric films.

Chapter 4 deals with the attenuation of ultrasound in dielectric crystals, that is, by scattering from thermal phonons. The topic is lucidly developed, and the importance of key features such as elastic anisotropy and thermal phonon lifetime is carefully explained. This is followed by one of the more controversial chapters, on the propagation of ultrasonic waves in ferromagnetic materials. There are two criticisms to be made of this section. Roughly half the chapter, about twenty pages, is devoted to an unnecessary review of the spin wave theory of ferromagnetism. Furthermore, most magnetoelastic experiments have been carried out at frequencies below 1 GHz and it is not clear that microwave ultrasonic studies have contributed greatly to the subject.

The converse is true of acoustic paramagnetic resonance, a field the development of which has been made possible only as a result of the advances in microwave ultrasonics, and also one

with which the authors have been closely associated. For this reason, perhaps, the chapter is particularly comprehensively written, with details not only of relevant crystal field theory and the spin-lattice interaction, but also of such relatively esoteric topics as rotatory dispersion and the phonon maser.

The amplification of ultrasound that can take place in some semiconductors is also treated in detail, with a useful discussion of non-linear and acousto-electric effects. Metals are mentioned only very briefly, on the questionable grounds that this is a low frequency topic. Mechanisms of ultrasonic attenuation by electrons are summarized and referenced, including those present in superconductors. The final chapter reviews the interaction of light with microwave ultrasound as studied by the techniques of Brillouin and stimulated Brillouin scattering. There are several useful appendices, mainly of crystal matrices and coefficients. Inevitably, certain sections of the book are already out of date, and the references are complete only up to 1970.

The book has been written at the first or second year graduate level and the mathematical formalism is presented in a clear and helpful manner. It is perhaps fashionable to criticize the price of books; nevertheless the figure of £17 cannot be allowed to pass without comment, because it is likely that those for whom the book is intended will not be able to afford it.

J. K. WIGMORE

Non-magnetic Metals

Introduction to the Theory of Normal Metals. By A. A. Abrikosov. Translated by Alex Baratoff. Pp. xi+293. (Academic: New York and London, 1972.)

AS is apparent from the title, unlike

other supplements to *Solid State Physics*¹, this is not a scholarly treatise on a specialist topic. Rather, it is a very unusual introductory text to the theory of non-magnetic and non-superconducting metals. Roughly speaking, it consists of a rapid sequence of "order of magnitude" calculations pertaining to the physics of normal metals.

Conventional topics like energy bands, Fermi liquid theory and scattering processes which give rise to resistance are quickly disposed of in the first four chapters. The rest of the book, ten more chapters, is devoted to problems which only recently have been explored in any detail. Many of these, like the various size effects, helicon waves, magneto plasma waves and Fermi liquid effects, are not yet part of standard texts on metal physics.

The calculations are short—the discussion is kept to a minimum—they are frequently novel, and always instructive. Using only the most elementary means, Schrödinger's equation, Boltzmann equation, and so on, the author contrives to make first principle calculations of such complicated effects as the giant oscillations in ultrasonic attenuation and the like, always including sufficient detail to enable the reader to follow each mathematical step. At the end of each calculation numerical estimates are given of the absolute magnitude of the effect.

There is no attempt at rigour. The aim is always to isolate the main contribution and to exhibit why the effect is interesting from the point of view of the basic interactions. On this level the book is an intellectual *tour de force*.

As the author warns the reader in the preface, the topics covered reflect his research interests. These are largely that of a many-body theorist and show up to an adverse effect in the rather misleading summary of the current state of band theory in chapter 14.

Nevertheless, it should make an interesting and an edifying reading for any solid state physicist, experimentalists and theorists alike. It should also be useful as a text in a more specialized course on metal physics for postgraduate students or as an outside reading for honours students doing solid state physics.

B. L. GYORFFY

¹ Ehrenreich, H., Seitz, F., and Turnbull, D. (eds), *Solid State Physics* (Academic Press: New York and London).

Angle on Aromaticity

The Aromatic Sextet. By Eric Clar. Pp. x+128. (John Wiley: London and New York, December 1972.) £1.50.

THREE groups of people should buy a copy of this book. Anyone who works with polynuclear systems will find in its pages an astonishing amount of factual detail; any theoretical chemist interested in electronic spectra or aromatic molecules should buy a copy and find a life's work of unsolved problems; anyone who knows Clar, or who has ever worked with him, will find here the familiar uncompromising interpretations of the experimental results which he has so indefatigably compiled.

Yet there will be few undergraduate courses that will adopt it, because so much of the theory in it offends against the accepted framework of bonding and aromaticity which all chemists (except Clar) subscribe to. Is he a latter-day Copernicus, and are we all wrong?

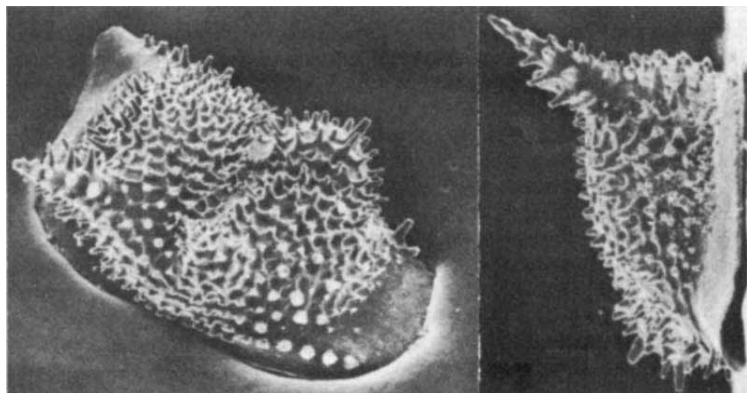
Clar considers the familiar circle inside the formula for benzene as an almost inseparable set of six electrons—Robinson's aromatic sextet. In naphthalene therefore only one ring at a time can be truly benzenoid (containing a sextet)—the other ring only possesses four π electrons! Anthracene likewise can only have one ring benzenoid at a time, whereas phenanthrene can have two benzenoid rings, leaving the 9,10 bridge with a localized double bond.

Clar goes on to treat more complex hydrocarbons in the same rigorous way, counting the number of rings which can simultaneously contain the magical aromatic sextet. When this is done, he can offer rationalizations of bond localization, chemical shift values, coupling constants, reactivity, and electronic spectra. These empirical computations might be christened "Clar's rules" to take their place with the Woodward rules of ultraviolet, or the Shoolery rules of nuclear magnetic resonance spectra.

What is needed for the moderates among us is a companion volume written from the standpoint of theoretical orthodoxy—but which is equally successful in explaining the facts. To my knowledge this will be a long time in coming.

WILLIAM KEMP

Ostracods in Stereo



Ostracod shells from *A Stereo-Atlas of Ostracod Shells*, by P. C. Sylvester-Bradley and David J. Siveter (Department of Geology, University of Leicester, 1973). The atlas is designed specifically to meet the needs of palaeontography, in which the study of fossils is strictly descriptive rather than interpretative.

Biological Sciences

Circulatory History

The Heart and the Vascular System in Ancient Greek Medicine: From Alemaeon to Galen. By C. R. S. Harris. Pp. ix+474. (Clarendon: Oxford; Oxford University: London, February 1973.) £15.

HARVEY'S discovery of the circulation of the blood (1628) was not made in a vacuum. It truly was a Copernican revolution. As such it provided a completely new vision against a *corpus* of time-honoured observations and interpretations. This had essentially not changed since having received its final form and codification by Galen in the second century AD. Indeed it was a formidable array of shrewd observations mostly anatomical and even experimental, and of theories as well as perfectly rational and methodical argumentation. It had seemed to serve well physiology, medicine and natural philosophy. New anatomical discoveries in the Renaissance, however important, had remained on the level of detail and failed to bring about the crucial break-away from the Galenic view. In this view the origin and motion of the blood had been decentralized and the liver sent venous blood to the periphery in a way analogous to the production of arterial blood by the heart. And there the matter rested. *Ex post facto* we may wonder why the ancients and their millenary successors did not arrive at the correct view. Before doing so, however, we require exact knowledge as to how far they had advanced in reality. In spite of the voluminous literature concerning Harvey and his predecessors no such comprehensive critical and detailed account has been available and wild claims on behalf of Galen have been allowed to pass. This unsatisfactory situation has now happily changed through the truly illustrious work under notice. Based on hundreds of *loci* (translated and given in the full original in the footnotes) the merits and failures of the ancients (and this implies of many an author up to Harvey) are critically weighed and presented in a crisp and clear style enlivened by delightfully humorous asides. Two outstanding parts of the narrative deserve special mention: Aristotle's three "ventricle" theory of the heart and Galen's finds and ideas concerning the pulmonary transit of blood. Why Aristotle should have assumed the heart to consist of three "ventricles" was explained elegantly and convincingly by T. H. Huxley (1880) and more recently by Erich Mühsam (1910) in observational terms. Both concluded that the whole of the right heart should have impressed

Galen as one, the left ventricle as the second ("middle") and the left auricle as the "third" cavity of the heart. For this observational interpretation the reviewer calls to witness no lesser Aristotelians than Harvey himself and Conring. Nevertheless the author finds contradictions and inconsistencies when all *loci* are taken into account. He would rather see in Aristotle's thesis the expression of his pre-occupation with "the mean" (*mesotes*)—a principle paramount in Peripatetic natural philosophy. Phenomena were then reduced to the interaction of opposites (*enantia*) possessed of a common "mean". It seems to me that the objections, though unanswerable in detail, neither detract from the interpretative value of the anatomical explanation nor make the latter incompatible with the metaphysical argument so aptly adduced by the author. Both would seem to entitle Aristotle to what may impress us as an advance rather than "error" when duly interpreted in context. No doubt can any longer remain of Galen's ignorance of the (misnamed) "lesser circulation". He was aware of the intra-pulmonary transit of venous blood into the arterial channels, but made no provision for its further progress into the left auricle. This admitted air alone and that for cooling and not for heating. Nor is there room for any "solution" of air in blood. With the clear demonstration of the bare facts culled from all the twenty odd volumes of the standard edition of Galen (Kühn) the wasps' nest of modernizing misconceptions and almost juridical and misleading pleas on Galen's supposed anticipation of Harvey and his immediate anatomical predecessors has been swept away. Indeed Galen was a conservative and never had any idea of blood circulation—neither of the so-called "lesser" nor of the "greater"—systemic—circulation. The reasons here given are: (1) the mis-conceived nature of the pulse and its connexion with the movements of the heart; (2) the view of the heart as centre of the respiratory system; and (3) the appraisal of the blood as a nourishment that was continually consumed in the periphery. In spite of his advanced anatomical knowledge, his insight into the propulsion of blood into the lungs as well as into the arterial system and the connexion of arterial and venous channels through capillaries he failed to notice the venous valves—a structure which assumed first-class dignity in Harvey's *inventio*. It should not be forgotten, however, that in some points of detail Galen had held more correct views than Harvey—the latter's disregard of the differences between the

arterial and venous blood, as emphasized by Galen, his adherence to the Aristotelian principle of the singleness (*henotes*) of the blood was a *felix culpa* which not unlikely promoted his discovery. Nor was Galen's belief in active "attractive" powers of diastole of the right atrium as wrong as Harvey thought. And finally we should not forget ideological reasons such as again the Aristotelian monocentricity of the blood as located in the heart (in contrast to its Galenic decentralization) which must have impressed Harvey, the staunch Aristotelian. Above all and overriding there was Harvey's quantification—it provided the initial "hunch" for the discovery which started as an idea and challenge to be proved by empirical evidence at this stage still wanting.

Here, then, we have a definitive work through which misleading seals and obscurities have been removed in a field of perennial significance for the historian, the physiologist and biological scientist at large. It is not a "bedside" book, to say the least, but a classic calling for careful and detailed study. It will amply reward its perusers in all future.

WALTER PAGEL

"Seeing" with Skin

Brain Mechanisms in Sensory Substitution. By Paul Bach-y-Rita. Pp. xiv+182. (Academic: New York and London, September 1972.) \$9.75.

THE first chapter of this book and parts of the second describe the equipment designed by Dr Bach-y-Rita and his colleagues for the conversion of optical images into patterned vibratory or electrical stimuli that can be applied to the skin. Part of the fourth chapter and a small part of the sixth describe results obtained with the equipment, chiefly on highly trained blind subjects.

The remaining two-thirds of the book is a review of some topics in sensory neurophysiology. This review is accurate, and contains some small original ideas; but there is very little connexion between it and the sensory substitution part. I was unable to find a single conclusion about brain mechanisms drawn by Dr Bach-y-Rita from his observations on sensory substitution; nor was I able to draw any myself. I could find only very few places in the book where facts discovered by physiological methods are used in interpreting the observations on sensory substitution, and in these few places the facts cited are well known and their application obvious; if the interpretative remarks had been omitted, they

could have been supplied by almost any reader.

The real importance of Dr Bach-y-Rita's work is not in the elucidation of brain mechanisms, to which it has not yet contributed anything and does not look very likely to contribute anything in the future. The field in which the work clearly may, if it succeeds, be immensely important is that of helping blind people. In this field, I think the ultimate criterion of success is clear: an invention has succeeded only if many blind people procure it at cost to themselves in money or inconvenience and then persist in using it as part of their everyday living. Dr Bach-y-Rita does not seem to claim to have met this criterion yet, but he would probably accept it as the right ultimate one. Where he and I probably differ is on criteria for partial success. I think a device has achieved significant partial success only when it has enabled a blind person to perform, better than he could without the device, an objectively assessable task of a kind that many blind people want to perform. Reading ordinary print or handwriting is such a task. A device for converting letters into auditory signals, the 'Optophone' of E. F. d'Albe, allowed a trained blind person to read ordinary print as early as 1917, but never met the ultimate criterion of success because reading by the auditory signals was slow, and learning to do it was very difficult. The 'Optacon' of Linvill and Bliss, which converts letters into tactile signals read with the fingers, allows faster reading with less preliminary training, and has nearly (perhaps quite) reached the ultimate criterion of success. Another objectively assessable task is to avoid obstacles, including moving ones, in walking. Any new device for this purpose has a formidable competitor in the guide dog.

Dr Bach-y-Rita concerns himself very little with the performance of his subjects in such objectively assessable tasks. There is one sentence on the reading of letters on page 90 which indicates a performance about ten times slower with his equipment than is commonly achieved with the 'Optacon', and a qualitative remark about the reading of graphs on page 152; beyond these I can find nothing. The attitude of the present book is well expressed in a sentence on page 156: "The theoretical basis for the design of the vision substitution system described above implies a concept of a brain so malleable that the subjective experience of 'vision' (as well as the quantitative and qualitative afferent information necessary for useful 'vision') could be obtained through an artificial receptor projecting to the cutaneous receptors".

Many blind people have vivid visual imagination, and describe themselves as "seeing" things that they feel with their

hands or with a stick. I shall be surprised if different visual substitution systems that are similar in the amount of information they convey prove to differ much from each other in the capacity for evoking such "seeing". The relative merits of any two proposed systems will be best measured by their capacity for conveying useful information in a non-technical sense; but information in the strict mathematical sense will surely be better than anything so intangible as whether the patient describes himself as seeing.

Devices that stimulate central parts of the visual pathway have a different status in this respect. We know from observations on sighted subjects that the sensation that they produce does not merely somewhat resemble vision; it is absolutely indistinguishable from it. Yet even here the important consideration is not the kind of sensation, but the efficiency with which it conveys useful information. In this respect, implanted stimulators of the visual cortex have not yet done as well even as Dr Bach-y-Rita's apparatus, and the 'Optacon' at present altogether outclasses them.

G. S. BRINDLEY

Developmental Metabolism

Critical Variables in Differentiation. By B. E. Wright. Pp. 109. (Prentice Hall: Englewood Cliffs, New Jersey and Hemel Hempstead, 1973.) £4.

THIS is both an interesting and a disappointing book. Its theme is that most developmental biologists are pursuing the wrong goals. Instead of looking at gene activation and rates of enzyme synthesis during development they should concentrate on the role of substrates and products and the control of metabolism by their complex interplay. This theme the author states forcibly and well. The book is disappointing, however, when it comes to the evidence.

The book falls into four parts, beginning with a review of selected data in the literature that confirm the author's contention that observed changes in enzymic activities in differentiating tissues need not be "critical variables" on which the rate or course of development depends. The second section starts with a description of carbohydrate metabolism during differentiation in the cellular slime mould *Dictyostelium discoideum*. The author reviews the enzymes that have been studied in this organism by various workers and then taking them one by one attempts to discredit their role as critical variables in development. The third section then presents a model of the metabolism of carbohydrate in *Dictyostelium* that is sufficiently simplified to be programmed into a computer using a specialized

"metasim" program. The computer duly predicts that the critical variables in nearly every case are not the enzymes that people have investigated but their substrates or products. The last section is concerned with speculation about the evolution of closed developmental systems. This section seems to be out of place in this book and will not be further discussed.

The book is generally well written and the arguments are lucidly presented. The diagrams are clear and helpful with the notable exception of Fig. 5, which is so involved that it looks as if it is intended to be a mental maze.

My main criticism concerns the use of the simplified metabolic model coupled to a computer program, which seems to lead to unwarranted conclusions based on numerous questionable assumptions. For example, on page 86 it is concluded that "in the case of UDPglucose pyrophosphorylase, although the enzyme activity and reaction rate increase together during differentiation, the increased enzyme level is not a primary critical variable—that is, it is not directly responsible for the enhanced reaction rate". The evidence for this comes from a computer analysis of a model of metabolism (page 58) that supposes that glycogen synthesis and degradation are in a closed cycle which is at steady state equilibrium at the start of development. Of the rate constants for the three reactions, (1) the synthesis of glycogen from UDPglucose, (2) the breakdown of glycogen to glucose-1-phosphate and (3) the synthesis of UDPglucose from glucose-1-phosphate, only the latter is known. The others were "found by assuming that at aggregation no end-product saccharides are synthesized and that a steady state exists; that is, the rate of UDPglucose synthesis equals the rate of soluble glycogen synthesis equals the rate of glucose-1-phosphate production". With these assumptions the author then demonstrates with the aid of a computer that increasing the activity of the enzyme UDPglucose pyrophosphorylase, which catalyses the third reaction above, does not increase the amount of UDPglucose formed but merely reduces the already small steady-state level of glucose-1-phosphate still further (page 61). This result is inevitable, however, because of the assumption made in the model of a fixed rate of glucose-1-phosphate production from glycogen. If glucose-1-phosphate is not made to be limiting then the computer shows that more UDPglucose is formed without raising the activity of UDPglucose pyrophosphorylase, but nowhere does the author show the computer prediction of the case in which UDPglucose pyrophosphorylase activity is increased along with an increase in glycogen

breakdown to maintain the levels of glucose-1-phosphate at those the author actually observes *in vivo*. In such conditions the activity of UDPglucose pyrophosphorylase would clearly have been shown to control the extent of UDPglucose production.

This and other parts of the model also depend on the highly questionable assumption that none of the metabolites whose concentrations are calculated are at all compartmentalized (that is the amount of metabolite measured in the cell is divided by the total cell volume to get the concentration *in vivo*). The notion that compartmentalization can be allowed for by finding the effects of altering the substrate/ K_m ratio of a particular enzyme (page 64) seems also unsound particularly because the K_m is altered rather than the substrate concentration.

This book is thought provoking and the principles expressed may well be very important, but the simplified metabolic models that are used are not worthy of the principles invoked.

PETER C. NEWELL

Environmental Physiology

Physiological Adaptations: Desert and Mountains. Edited by M. K. Yousef, S. M. Horvath and R. W. Bullard. Pp. xiv+258. (Academic: New York and London, September 1972.) \$14.50. *Life, Heat and Altitude* by D. B. Dill was published in 1938 and has continued to influence physiologists. Today, Dr Dill is actively working at the Desert Research Institute, Boulder City, Nevada, where a symposium was held in celebration of his eightieth birthday; this volume is a record of the proceedings. Many of the contributors were associated with Dill at the famous Harvard Fatigue Laboratory, and inevitably some papers emphasize experimental work in which they collaborated with Dill, but the volume is far more than a collection of physiological reminiscences or personal tributes. There are twenty-six chapters, with an impressive list of authors, all distinguished for work on environmental physiology. It is not possible to review each chapter; only a few aspects can be mentioned. E. F. Adolph, himself an active veteran of environmental physiology, has brought up to date his ideas on "general concepts of physiological adaptation", which he summarizes as "superfunction is an end product of physiological adaptation". R. A. McFarland, who was also a member of the Harvard Fatigue Laboratory, has restated his views on the role of oxygen in the process of ageing, revised in the light of recent work on the effects of altitude. H. S. Belding discusses the mechanisms of acclima-

tion to heat, and emphasizes the term "quickening" as exemplified by the more rapid onset of sweating in the acclimatized compared with the unacclimatized individual. A. P. Gagge writes on "Partitioned Calorimetry". This is a really valuable contribution, not so much because of novelty as in the clarity and precision with which a difficult subject is presented.

L. D. Carlson, whose recent death has deprived us of one of the most thoughtful and penetrating workers in this field of physiology, in a "speculative analysis of certain aspects of adaptation", points out that desert animals are not exposed to a constantly hot environment but regularly encounter the heat of the day and the cold of the night. Furthermore, "many desert animals live in the mountains nearby as well" and hence may need to adapt to altitude as well as heat and cold. Carlson uses many of Adolph's concepts, rewording them as, for example, "A biological

system is inconceivable without its external environment". He discusses "responses that may be time-dependent within the time range of day-to-day existence" and wonders if the difficulties of interpreting climatic factors in evolution may be due to the use of average climatic conditions, instead of examining the effects of a range of conditions.

D. H. K. Lee's chapter "Large Mammals in the Desert" draws extensively (and rightly) on the work of Schmidt-Nielsen and his colleagues, but he also draws on his own work on man. He makes the interesting point that upright man at noon presents a minimal area for solar radiation, as indeed others have shown, but in contrast a quadruped such as a cow receives about the same large radiant load virtually throughout the day. "Small Mammals in the Desert" by W. G. Bradley and M. K. Yousef, is concerned with the "complex interlocking patterns of behaviour and physiology" which make it possible for

Nocturnal Insectivores



Dasycercus cristicauda (top) and *Sminthopsis crassicaudata* illustrate two different types of water economy found in desert animals sharing the same ecological niche. *D. cristicauda* has a low water turnover, *S. crassicaudata* a high one. The two animals are illustrated in the chapter by W. V. Macfarlane and B. Howard in *Comparative Physiology of Desert Animals*, edited by G. M. O. Maloiy (Academic Press, New York and London, 1972).

large numbers of small mammals to flourish in the desert conditions of temperature extremes, water scarcity and food shortages. The behavioural aspects of temperature regulation have been neglected by physiologists until recently; the desert animal provides opportunities for studying behaviour as a physiological component of a range of adaptation.

Articles by W. H. Forbes, B. Balke, R. W. Bullard, C. F. Conzalezio and S. M. Horvath deal with various aspects of adaptation to altitude. Although in most of the book there are few reports of new work, the individual writers have taken the opportunity to have a new look. The results are varied but are stimulating because of a number of unexpected and exciting ideas.

O. G. EDHOLM

Adaptation in the Sea

Environmental Physiology of Marine Animals. By Wiona B. Vernberg and F. John Vernberg. Pp. ix+346. (Springer: New York and Berlin, 1972.) 62.40 DM; \$19.80.

THE purpose in writing this book was to bring together in a text for college and graduate students the main concepts encountered in the physiology of the marine environment. The result is a concise, readable attempt to describe the functional responses of marine animals to alterations in the environment. The first chapter is concerned with definitions related to the functional processes in organisms, to detection by the organism of changes in its environment and to the physiology of adaptation. Some interesting aspects of the duration of exposure to stressful environmental factors are mentioned briefly. There follows a condensed account of the characteristics, chemistry and physical properties of sea water which also gives definitions involving the relations of organisms to temperature, pressure, tides, salinity, light and other factors. The marine environment is considered subsequently in four main sections: on the intertidal zone, the estuarine environment, the coastal and oceanic environment and the deep sea. After a brief introduction to each section outlining some characteristics of the zones, responses by marine organisms are dealt with under the main headings of resistance adaptations and capacity adaptations. The former include tolerance to changes in temperature and salinity, to desiccation and oxygen stress. Capacity adaptations are dealt with under headings such as perception of the environment, feeding and the related morphological and physiological mechanisms which ensure extraction of the necessary energy from the environment, how there is a metabolic response and what is des-

cribed as a physiological strategy reflected in oxygen utilization. Useful sections consider how circulatory systems react to stress and review osmoregulatory activities in respect to habitat preference. Reproduction and development are influenced in each zone by factors including food, salinity, temperature, oxygen, photoperiod and lunar cycles. The arrangements in the book of zones, responses and the various factors operating in the environment enable a reader to find his way to what he wants with ease. The chapter on the deep sea contains interesting material but was written before contributions at a recent conference (*Symp. Soc. Exp. Biol.*, 26; 1972) on the effects of pressure on organisms could be included. The last chapter deals with pollution effects; thermal aspects, radionuclides, industrial wastes, pesticides are discussed and a striking electronmicrograph shows the damaging effects on the gill tissue of fiddler crabs after they have been maintained in 0.18 p.p.m. Hg for 6 weeks. A final short section considers the possible far reaching consequences of human manipulation of the marine environment. Successful aquiculture can only develop when our knowledge of the adaptations of potential food species will allow management and selection similar to those that helped agriculture. The text is usefully illustrated with numerous clear figures, tables and diagrams. It presents much information, occasionally in a somewhat staccato and condensed form but which cannot fail to be valuable to anyone studying physiological responses in marine creatures, even though as the authors themselves admit we do not yet understand why an organism lives where it does.

R. J. HARRISON

Eating in Old Age

Nutrition in Old Age. Edited by Lars A. Carlson. Pp. 180. (Almqvist and Wiksell: Stockholm 1972.) Sw.kr. 50.

AS stated in the introduction, few nutritionists are interested in the problems of old age and few gerontologists are interested in nutrition. This international symposium brings together some of the information available regarding the nutritional needs of the elderly and underlines the shortage of information, while at the same time emphasizing the difficulties in collecting evidence.

Dr W. T. C. Berry points out that epidemiologists generally require a minimum of 1,500 subjects while several of the papers indicate that comprehensive measurements of a few hundred elderly subjects call for a major effort. It is difficult to measure even food intake with any accuracy and much more difficult to establish the presence of mild degrees of undernutrition either

in individuals or groups. Body weight can be very misleading because of changes in the ratio of fat to lean body mass with advancing years. Certainly it is well known that energy output falls with age so that there is likely to be a fall in the intake of nutrients.

On the basis of work carried out both longitudinally on 650 males over the past 10 years, as well as by cross-sectional analysis, Dr N. W. Shock concludes that the fall in basal metabolism with age is simply a reflexion of the reduced amount of metabolizing tissue. His colleague Dr R. Andres shows that ageing results in reduced glucose tolerance, but there is no evidence on which to decide whether this is due to the appearance of diabetes or a non-pathological manifestation of ageing.

Drs Werner and Hambraeus of the University of Uppsala have investigated the digestive capacity of 700 old people and reported intolerance to fat and protein in amounts well tolerated by younger people, not associated with disease of the digestive tract. It is not clear whether this is due to decreased production of digestive enzymes or reduced absorption.

The average bone loss between middle life and old age is 15% of the skeleton. Senile osteoporosis, which affects both sexes from the age of 60, is associated with declining absorption of calcium and is partly reversible with vitamin D. Dr B. E. C. Nordin and his colleagues state that mild vitamin D deficiency causes malabsorption of calcium leading to osteoporosis, while more severe deficiency lowers the plasma calcium and leads to osteomalacia. An interesting suggestion was made during the discussion that evidence of the preventive properties of vitamin D might be derived from comparison of these bone disorders in the United States, where liquid milk is fortified with vitamin D. But in support of the contention that so little information is available in this whole field it was stated that fracture rates as a function of the population at risk are available only from the UK and Sweden.

The evidence for increased requirements of vitamins with old age is reviewed by Dr Sylvia Darke, who concludes that there is none. The difficulties are illustrated by the finding that while anorexia, insomnia and increased irritability are associated with early stages of thiamin deficiency and are common complaints of the elderly there is no evidence of a connexion.

One problem discussed by Drs Soremark and Nilsson is that of decreased taste acuity, salivary flow and loss of teeth which have a major effect on dietary selection, even if incomplete mastication has no effect on efficiency of digestion.

The final discussion where speakers

point out necessary fields of further research and unsolved problems is an extremely valuable part of the book. One of these problems is the association between longevity and restricted food intake; another is the longer life of women—the discrepancy in the US has risen from 2 years to 8 years since 1900 in spite of the greater prevalence of obesity in women (and the observation that obesity shortens life). Above all is the major problem of the relation between unusual or so-called abnormal biochemical parameters of nutritional status, health and longevity. What is "normal" has long been undefined: the greater divergences from average met in the elderly make the question even more difficult to answer.

ARNOLD E. BENDER

Gene Mapping

Gene Mapping in Laboratory Mammals. Part A. By R. Robinson. Pp. vii+151. (Plenum: London and New York, 1971.) \$17.

THIS book, the first of a two-part work, represents a bold attempt to summarize the mathematical and statistical aspects of linkage estimation; but its usefulness must be questioned. For those with a modicum of mathematics, it could reasonably be argued that the existing

literature is adequate. The bibliography of the present volume cites books on the mathematical aspects of linkage, and provides an entry to the primary literature, which is generally quite accessible. There is, however, a need to present the ideas to those without mathematical sophistication but who may have genes to map. Such people will not profit at all from this volume. Unless they understand maximum likelihood, for example, they become drop-outs on page 24, without learning even what it is all about. The introduction of matrices on page 29 is equally pre-emptory. In general, the mathematical presentation, though mostly elementary, is very inexplicit, and anyone with sufficient knowledge to follow the development of the topic in this book is at least well enough equipped to get along without it.

The book has its odd moments, as on page 36, where a simple statistical identity is credited to a well-known geneticist. And in a volume implying much tiresome computation, it is incongruous to find that Table 4.1 only lists some values of a simple function (badly misprinted in the title of the table) which is perfectly amenable to pencil-and-paper arithmetic. And so forth. In a more positive vein, the author draws attention to the difficul-

ties of estimation when normal segregation is disturbed. This, as he points out, is a problem in practice though, I should have thought, one that is widely appreciated. What the uninitiated would have welcomed would be a clear exposition, in terms of worked numerical examples, of the methods of coping with messy data. Unfortunately, this volume has only one numerical example, and even that is hypothetical and trivial; it suffers further from a confusing misprint. Hopefully, more worked-out examples will be given in the forthcoming part B, which promises to examine "the accumulated data on linkage for most of the laboratory mammals and provides a comprehensive and up-to-date survey". The second part will therefore be eagerly awaited by many, and if its price is kept down because part A has been issued separately, then we need not question too closely a publication policy that yields two volumes where one might have done. R. C. ROBERTS

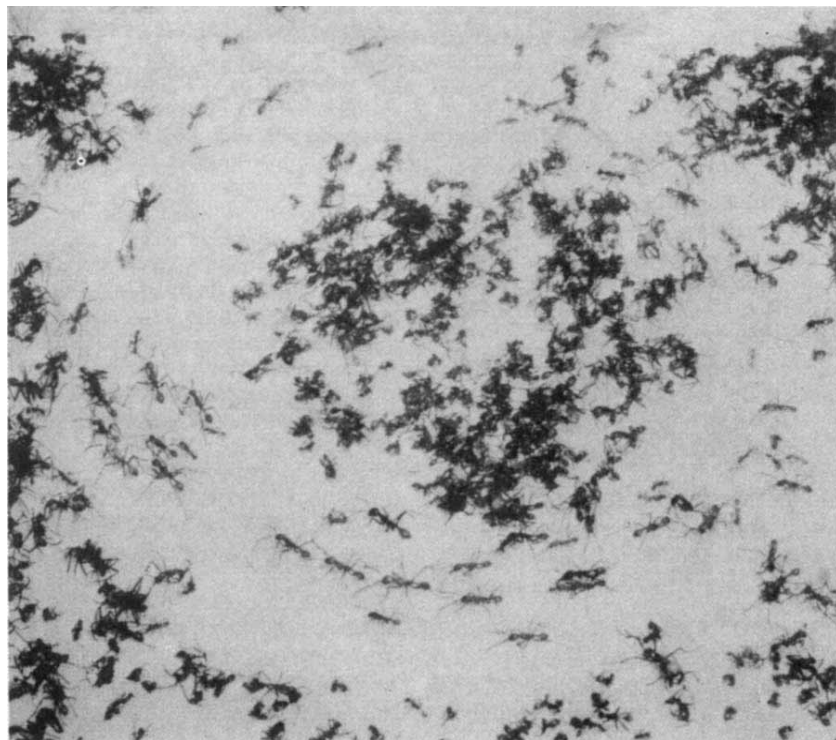
Membranes and Impulses

Perspectives in Membrane Biophysics. Edited by D. P. Agin. (A Tribute to Kenneth S. Cole.) Pp. vii+317. (Gordon and Breach: New York, Paris and London, 1972.) £7.70.

THERE can be no room for doubt that the work of K. S. Cole has been of seminal importance in the field of nerve and muscle biophysics. It was he and Curtis who first showed in 1938 that the nerve impulse is accompanied by a large increase in membrane conductance. In 1949 he and Marmont gave the first descriptions of the voltage clamp technique that formed the basis of Hodgkin and Huxley's classical analysis of the impulse mechanism in 1952. Over a period of 40 years he has influenced a large number of the most important workers in the field. This volume contains sixteen papers written as a tribute to Cole on his seventieth birthday by former colleagues, students or admirers.

Some of the papers are based on work already published or take the form of a normal journal or review paper. In addition, however, there are some gems that have clearly been written specially for this volume because they concern particular problems that are uniquely, or almost uniquely, associated with Cole's name. To give two examples of these: R. D. Keynes discusses some reasons for investigating in more detail the relative constancy of the membrane capacitance established by Cole and Curtis in their impedance measurements; Adrian, Chandler and Hodgkin describe an elegant extension of what has come to be called "Cole's theorem" and its application to threshold measurements in skeletal muscle. These papers have very obviously been written in response to the invitation to contribute to this

Army Ants



Circular column of the famous army ants studied by T. C. Schneirla, whose selected writings, on topics from social bonds to conceptual trends in comparative psychology, and including much about ants and other insect behaviour, are published by W. H. Freeman (*Selected Writings of T. C. Schneirla*, W. H. Freeman, San Francisco, 1972).

volume. All the papers, however, draw their connexions to Cole's work in perfectly natural ways, including in many cases descriptions of the way Cole had suggested or influenced the work. One obtains therefore an unusual view of the way in which one man has contributed directly or indirectly to the development of the field. The result is a fitting tribute to Cole, quite apart from being a valuable collection of papers on membrane biophysics.

D. NOBLE

Heredity for Laymen

Heredity in Humans. By Amram Scheinfeld. Pp. xvi+303. (Chatto and Windus: London, January 1973.) £2.50.

THIS is the fifth, or, according to one's classification, the ninth book by Scheinfeld aimed at explaining heredity to the layman, which began in 1939 with the publication of the very successful *You and Heredity*. It is not intended to compete with the more voluminous and detailed *Your Heredity and Environment* of 1965, but may be regarded as an up-to-date successor to *The Human Heredity Handbook* published in 1956.

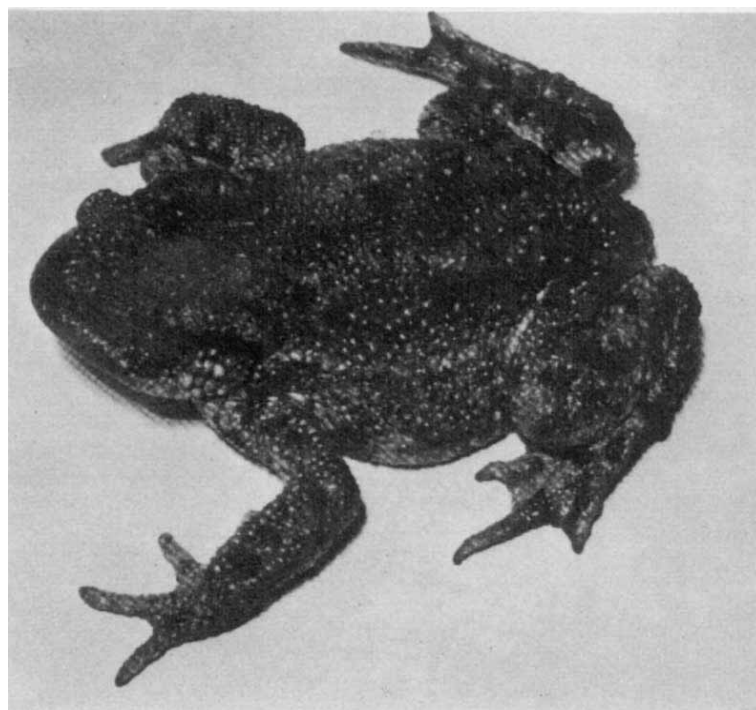
The approach is "pragmatic" rather than analytical or technical, attempting to answer many questions asked by laymen in laymen's language: for instance why children resemble their parents, but not always; in what way men and women "naturally" differ; how certain diseases and defects are inherited; how ethnic groups differ in their genes; what part heredity plays in intelligence, talent, behaviour, personality and sex life.

Most offered explanations are roughly correct, but the method of presentation is often unnecessarily crude and also too dogmatic. It is probably not very useful to describe dominant genes as "bully genes", genes producing various oddities as "freak genes", genes working unexpectedly and only under some circumstances as "temperamental genes"—genes of late onset as "Rip van Winkle genes". Some curious concepts also should be pointed out. Why, for instance, call some Indians, Arabs and North Africans "dark skinned whites"?

The situations used for the illustration of the author's points of view in respect of race, intelligence differences and similar problems are mostly chosen from the orbit of metropolitan New York and sometimes a certain bias seems to have prevailed; it is, for instance, curious not to mention the Germans among the people who contributed to music. Nevertheless, if laymen do not take the text as gospel—which, unfortunately, some of them tend to do—this book can do a good deal in counteracting antiquated and bigoted assertions.

H. KALMUS

Common Toad



A particularly large female toad illustrating the adaption of amphibians to relatively dry environments in *Amphibians*, by J. F. D. Frazer (Wykeham Publications, London, 1973).

Diseases of Plants

The Principles of Plant Pathology. By S. A. J. Tarr. Pp. 632. (Macmillan: London and Basingstoke, September 1972.) £10.00.

PLANT pathology, whether interpreted narrowly or as broadly as in this book, is to plants what the whole of medicine and veterinary science is to man and animals. It is therefore not surprising that Butler and Jones's monumental textbook *Plant Pathology* of 1949 is said to have marked the end of an era. Increasingly the subject requires several multi-author-volumes. The facts about individual pathogens, diseases and ways to control them seem best presented in loose pamphlets sold on a subscription basis, as some are through the Commonwealth Mycological Institute. In this way every topic can have specialist treatment and be revised separately whenever necessary. Review journals satisfy a further part of the need, but neither pamphlet nor review would provide the "thorough background to all aspects of plant pathology" which this book is claimed to do. The task was certainly ambitious because, with only a brief history of plant pathology written by Dr Ainsworth and several helpers who checked other chapters, Dr Tarr undertook work that recently

occupied three volumes of contributions by many authors. Struggling no doubt against severe limitations of space and price the author has produced an up-to-date book of pleasing format, with few errors but with illustrations that are rather scanty and often disappointingly presented.

Many apologies for brief accounts show that Dr Tarr knew how much he was omitting. Estimates of the soundness of his choice will probably differ depending on the interests and experience of his readers. So will opinions of the wisdom of mentioning rodents, birds and molluscs as if they, as well as some angiosperms and chemical excesses or deficiencies, should be regarded as plant pathogens. However, here as elsewhere, Dr Tarr plainly states the alternatives possible and the consequences that each may incur. He rarely obscures our ignorance under a heap of terminology, but occasionally invents terms for which the need is doubtful. Dr Tarr states that he has aimed his book at students, and novices will appreciate the wide scope of the book and be glad of the reminders that come in the rather repetitious and overlapping early chapters, some so abbreviated that they contain little but lists and generalizations. Surely a treatment of the major nutrients (NPK) that only

occupies two and a half pages and three references cannot do them justice. Nevertheless teachers will applaud the book for many references, some so recent that they were probably added in proof, and for the author's habit of quoting major references at the end of sections and of the book. Students lucky enough to borrow the book, for sadly few can afford personal copies, will find it an excellent "thought-saving" source for essays.

However, students grow into practitioners of plant pathology and they will often find the six hundred pages contain disappointingly brief answers to their inquiries or conclude their special interests have too little attention. Is it right that any comprehensive book on the principles of plant pathology should dismiss viruses and mycoplasmata with thirty-six pages and an apology? Although "root diseases" are mentioned often, the chapter devoted to them is only a third of that on viruses and about half that on "non-parasitic agents". The chapters dealing with infection, attack and defence mechanisms are more satisfying and the author is to be complimented on recognizing both that there is an epidemiology of root diseases and the importance of pathogen and host carry-over in starting epidemics. The latter theme is emphasized again through disease control by sanitation and cultural practices. Other chapters contain good summaries of existing knowledge on dispersal, forecasting and assessing diseases or controlling them by fungicides, resistance, quarantine and so on. However, I am still waiting for the book that not only describes the principles of plant pathology as an academic subject, but also stresses how disease control must be fitted into changing agricultural systems and show benefits greater than its cost.

J. M. HIRST

Chromatophores

Chromatophores and Color Change: The Comparative Physiology of Animal Pigmentation. By Joseph T. Bagnara and Mac E. Hadley. Pp. xvi+202. (Prentice-Hall: Englewood Cliffs, New Jersey, January 1973.) £6.

NOTWITHSTANDING the jacket's claim, "a balanced review of all facets", there is a strong bias towards the authors' own fields of research, as they are first to emphasize. Some readers will question whether so strong a bias is justified under the very broad title. Certainly the authors' research fields—the micro-anatomy and development of chromatophores, and their behaviour as effector organs in vertebrates—have been among the most progressive in

recent years and this series of books is intended for specialists as well as for students.

The relevant chapters account for 80% of the whole text (85% if the introductory and concluding chapters are excluded) and their share of the references is 78% (80%). They constitute admirable reviews but the remainder seem too sketchy even for junior students. Chapter 3 on the biochemistry of the pigments seems particularly light, and also adrift, no doubt because the "visual-effect" functions of the integumental chromes depend essentially on biophysical properties and mechanisms, chemical properties being largely irrelevant.

It should be re-emphasized that the structure and development of chromatophores and their organelles are well reviewed, with excellent illustrations and that the hormonal and nervous controls of vertebrate chromatophores are very critically discussed: some may feel that the authors are hypercritical concerning the significance of melatonin. There is a generous bibliography, mainly of recent references, in line with the aims of the series, but what a pity such a long list is not arranged alphabetically, particularly as there is no author index.

In spite of the weight of the authors' summing up, a number of leading questions must be considered still sub-judice. Many will wear Sir Lancelot's favours in the tournament of hormonal control and the albedo school will question whether a pineal-incident light response can be independent of the pituitary-background response.

A. E. NEEDHAM

Nobel Biology

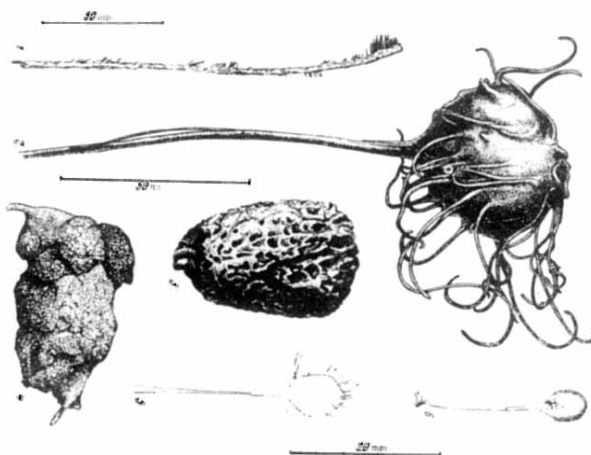
Nobel Lectures, Physiology or Medicine. 1963-1970, including Presentation Speeches and Laureates' Biographies. Pp. x+273. (Elsevier: Amsterdam, London and New York, 1973.) Dfl. 110; US\$ 34.50.

THESE Nobel Lectures are an enjoyable if expensive way of keeping up with the major advances of biology and medical science. Needless to say, the authors are an all-star cast—in order of appearance) Eccles, Hodgkin, Huxley; Bloch and Lynen; Jacob, Lwoff and Monod; Rous and Huggins; Granit, Hartline and Wald; Holley, Khorana and Nirenberg; Delbrück, Hershey and Luria; Axelrod, von Euler and Katz.

The lectures are about equally divided between molecular biology in its now accepted sense, expounded by many of its founders, and neurosensory physiology, a subject that was deeply molecular long before many of the disciplines now eager to be so described. The lectures are remarkably good reading because the authors have obviously taken considerable pains with them. The citation or presentation speeches have also been very carefully prepared and add greatly to the value of the volume. These Nobel Lectures could be really useful reading for students of medicine or life sciences from whom future laureates must be recruited. It is a pity that some great Foundation at its wits' end to know how to spend its money does not subsidize their publication in paperback, so putting the lectures within the means of those who would benefit most by reading them.

P. B. MEDAWAR

Marine Fauna



Inhabitants of the Kurile-Kamchatka Trench, as revealed in the thirty-ninth cruise of the Vityaz: one of the drawings from *Fauna of the Kurile-Kamchatka Trench and its Environment*, edited by V. G. Bogorov and distributed for the Academy of Sciences of the USSR and the Israel Program for Scientific Translations by Wiley (Chichester, 1973).

Continued from page 576

in the original diploid micronucleus. The macronucleus divides into two bodies to complete the process of macronuclear formation, and the cell resumes vegetative reproduction with a maximum cell cycle time of about 8 h.

The loss of DNA that occurs during the vesicle stage of the macronuclear anlage is apparently restricted to particular nucleotide sequences⁵. The buoyant density profiles of micronuclear and macronuclear DNAs are compared in Fig. 4. The density profile of micronuclear DNA has several peaks; a computer analysis has indicated that the profile is probably made up of a minimum of four density components with peaks at 1.699, 1.701, 1.704 and 1.709 g cm⁻³. In contrast, the profile for macronuclear DNA has a single component with a peak density at 1.701 g cm⁻³. Presumably the nucleotide sequences present in the macronuclear DNA are represented in the micronuclear DNA only by the component with a peak density of 1.701 g cm⁻³. Assuming the presence of four density components in the micronuclear DNA, the computer analysis indicates that the DNA with a density of 1.701 g cm⁻³ accounts for less than 10% of the total micronuclear DNA. The melting profiles of macronuclear and micronuclear DNAs show essentially the same phenomenon (Fig. 5). Macronuclear DNA melts as if it were a single component, whereas micronuclear DNA melts as if it contained several components of different base composition.

We conclude that the absence in the macronucleus of several density (or melting) components of DNA, which account for more than 90% of the total micronuclear DNA, must be a consequence of the observed destruction of 93% of the DNA in the polytene-like chromosomes. This does not necessarily mean that only 7% or so of the different sequences present in micronuclear DNA are retained in the macronucleus; some of the destroyed DNA might represent multiple copies of one or a few nucleotide sequences. The fact that much of the micronuclear DNA absent in the macronucleus forms discrete density components in CsCl density gradients suggests the pres-

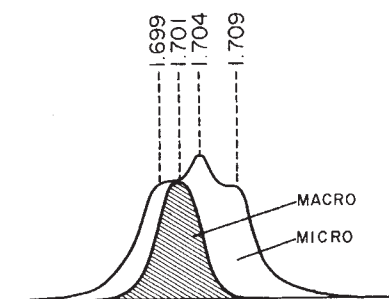


Fig. 4 Buoyant density profiles in CsCl for micronuclear DNA (above) and macronuclear DNA (below). Micronuclear DNA consists of at least four density components with peaks at about 1.699, 1.701, 1.704, and 1.709 g cm⁻³. Macronuclear DNA consists of a single component with a density peak at 1.701 g cm⁻³.

ence of repetitious DNAs, since these components might be analogous to the satellite DNAs demonstrated in many higher eukaryotes. In any case, the buoyant density and melting profile data indicate a major reduction, if not elimination, of some fraction of the micronuclear nucleotide sequences during formation of the macronucleus.

The most direct, quantitative way to measure the reduction in nucleotide sequences in the macronucleus would be to compare the renaturation rates after heat denaturation of the DNAs from the two kinds of nuclei. For several technical reasons micronuclear DNA is difficult to prepare from *Stylonychia* in adequate amounts for this kind of analysis, but it probably can be done. Macronuclear DNA is easily obtained, and this DNA renatures with a Cot 1/2 about twenty times greater than does the DNA of *E. coli* (unpublished results of D. M. P. and C. J. B.). The renaturation pattern for macronuclear DNA contains no indication of sequence repetition.

Nothing is known about the mechanism by which the destruction of DNA in the macronuclear anlage is achieved.

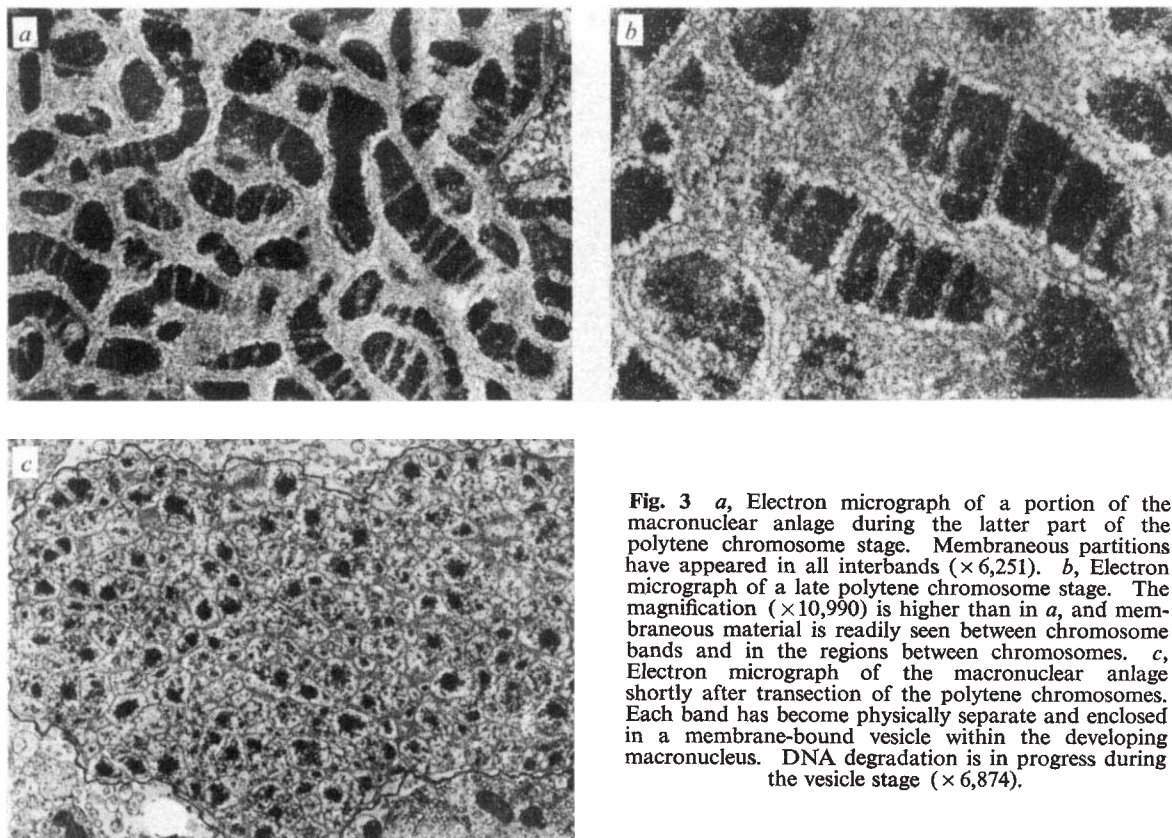


Fig. 3 *a*, Electron micrograph of a portion of the macronuclear anlage during the latter part of the polytene chromosome stage. Membraneous partitions have appeared in all interbands ($\times 6,251$). *b*, Electron micrograph of a late polytene chromosome stage. The magnification ($\times 10,990$) is higher than in *a*, and membraneous material is readily seen between chromosome bands and in the regions between chromosomes. *c*, Electron micrograph of the macronuclear anlage shortly after transection of the polytene chromosomes. Each band has become physically separate and enclosed in a membrane-bound vesicle within the developing macronucleus. DNA degradation is in progress during the vesicle stage ($\times 6,874$).

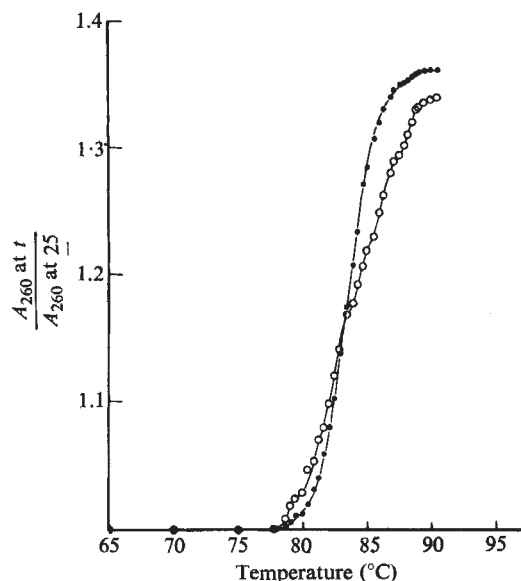


Fig. 5 Melting curves for micronuclear DNA (○) and macronuclear DNA (●). Macronuclear DNA melts with a sharp transition in hyperchromicity, indicating the presence of a single component. Micronuclear DNA melts with a complex pattern, indicating the presence of several components.

The elimination of DNA must be controlled in some way to assure the retention of essential nucleotide sequences because it is the macronucleus that runs the vegetative life of this cell. Nuclease action must therefore be specifically directed toward certain nucleotide sequences, but the basis of this specificity is completely unknown. This specificity apparently does not involve 5' methyl cytosine residues in DNA because neither micro- nor macronuclear DNAs of *Stylonychia* contain a detectable number of such residues (personal communication from D. Brown).

From the cytological standpoint the two simplest hypotheses to explain the elimination of DNA are as follows. (1) All the DNA in 93% of the vesicles is destroyed, and all the DNA in 7% of the vesicles is retained. (2) Each of the vesicles formed from the polytene-like chromosomes loses 93% of its DNA. It is not possible at present to rule out either of these hypotheses.

If the decrease in DNA in *Stylonychia* takes place by the degradation of 93% of the DNA in each band (vesicle), then the remaining 7% of the DNA might reasonably be expected to be of low molecular weight. The pieces of DNA produced by the transecting of the polytene chromosomes would be further reduced in size by the 93% degradation of DNA. Evidence that the macronuclear DNA is small has already

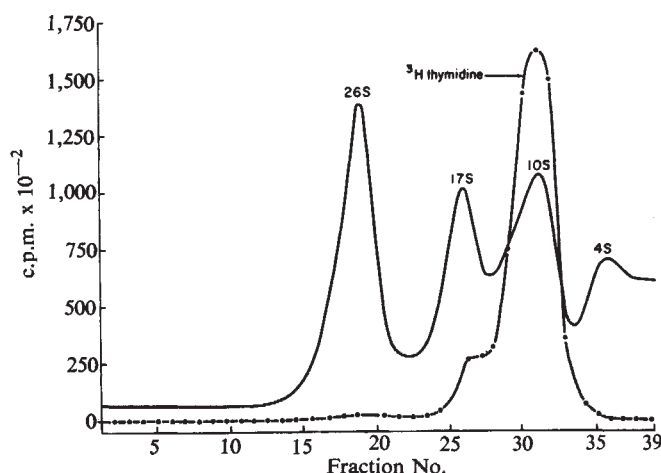
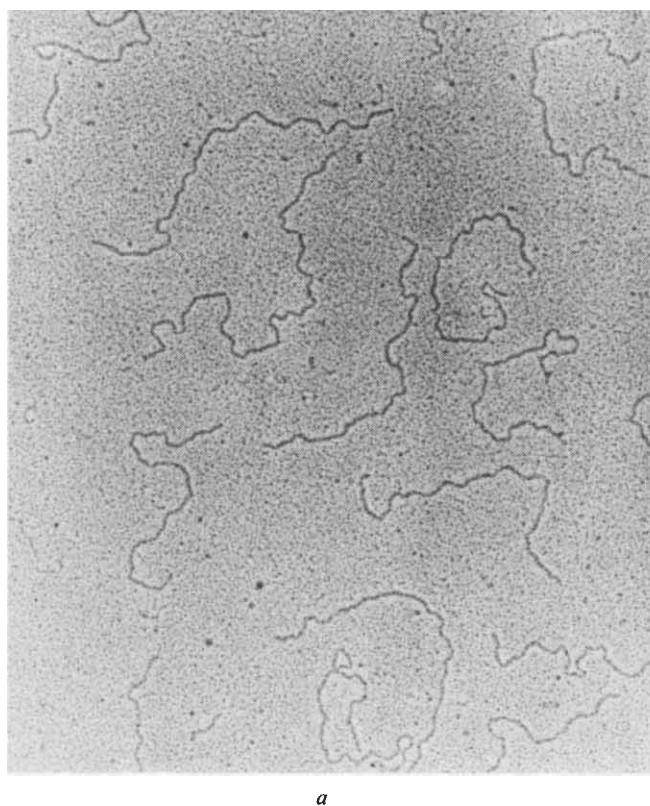
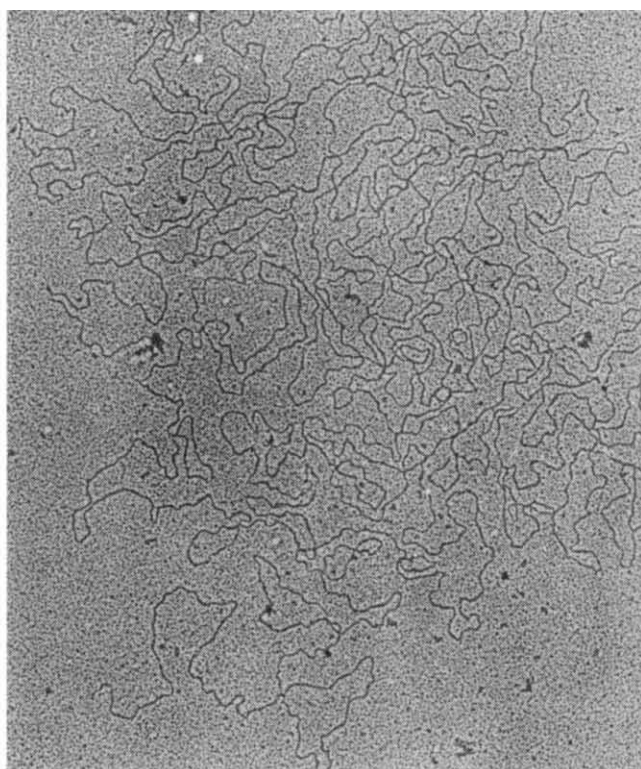


Fig. 6 Sucrose gradient sedimentation of nucleic acids from isolated macronuclei. The absorbance profile shows ribosomal RNA (26 and 17S), 4S RNA, and DNA (10S). The cells were labelled with ^3H -thymidine for several cell cycles, and all radioactivity is contained in the 10S peak plus a small 14S shoulder.



a



b

Fig. 7 a, Electron micrograph of macronuclear DNA. All the DNA is in short pieces, with an average length of $0.8\ \mu\text{m}$ ($\times 49,000$). b, Electron micrograph of micronuclear DNA. All the DNA is in long pieces, and free ends are scarce ($\times 20,000$).

been published⁶. First, macronuclear DNA sediments with a peak of about 10S in a neutral sucrose gradient (Fig. 6). The 10S peak has a shoulder on the heavier side, and occasionally this shoulder is resolved as a peak of 14S. Micro-nuclear DNA sediments with S values between 20 and 100 depending on the amount of shearing that occurs during preparation. Various tests (described in Prescott *et al.*⁶) indicate that the low S value for macronuclear DNA is not due to degradation during DNA isolation.

In the electron microscope, DNA from the macronucleus appears as small pieces from 0.2 to 2.2 μm long (Fig. 7a). In contrast, micronuclear DNA from the same cell lysate occurs in much longer pieces—usually too long to measure, since free ends of molecules are not common (Fig. 7b). The histogram in Fig. 8 shows the distribution of size of macronuclear DNA molecules. The distribution is bimodal with a major peak of DNA pieces with a mean size of about 0.75 μm and a minor peak of DNA pieces about 2.0 μm , the major and minor peaks presumably reflecting the two peaks (10S and 14S) observable in sucrose gradients.

The following information is available about the pieces of macronuclear DNA. When macronuclear DNA is heated, in the presence of formaldehyde, to the temperature at which hyperchromicity just begins to appear, every piece of DNA melts at one end⁷ (Fig. 9); no pieces have ever shown melting at both ends. The melting is interpreted to reflect a higher content of adenine and thymine in one end of each molecule than is present throughout the main part of each molecule. At the very least, this observation shows that each piece of macronuclear DNA has a polarity, and suggests that all pieces share a common property at one end.

The pieces of macronuclear DNA also bind RNA polymerase⁷. One molecule of RNA polymerase (from *Bacillus subtilis*) is bound at the end of each DNA piece when the binding reaction is performed in the absence of pyrimidine nucleotide triphosphates (Fig. 10). Binding has never been observed at both ends or at internal regions of molecules. This means that one end of each piece of macronuclear DNA has a high affinity for RNA polymerase. Whether the RNA polymerase binds at the end that is richer in adenosine and thymidine remains to be proven. Clearly the experiments

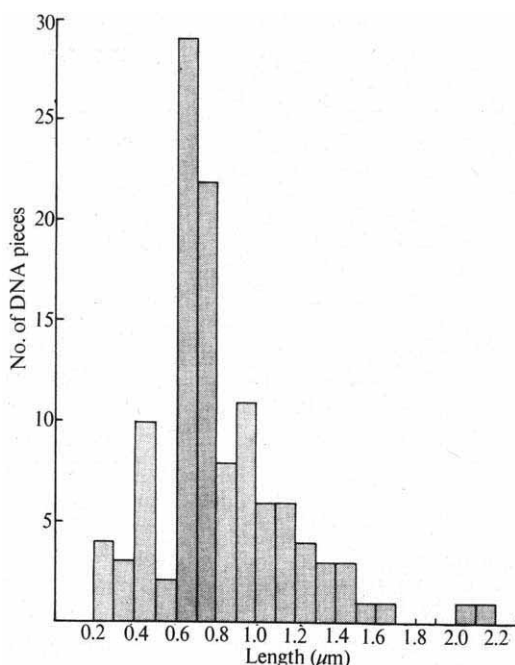


Fig. 8 The distribution of length of macronuclear DNA measured in the electron microscope. The larger pieces are believed to reflect the small 14S shoulder seen in sucrose gradients.

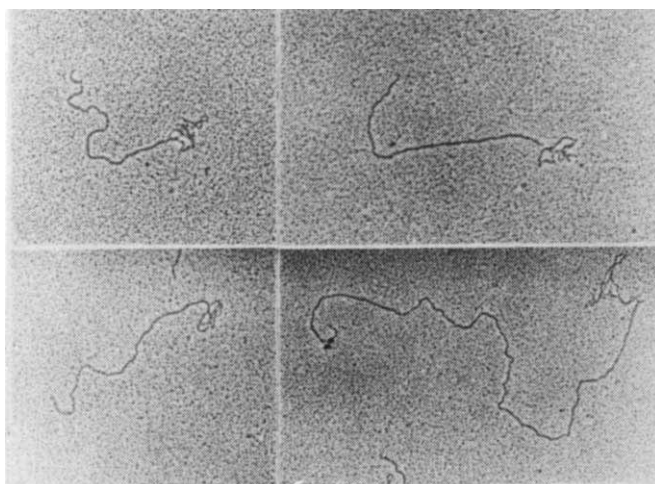


Fig. 9 Electron micrograph of macronuclear DNA brought to the beginning of melting in the presence of formaldehyde. Each piece of DNA has a melted region at one end.

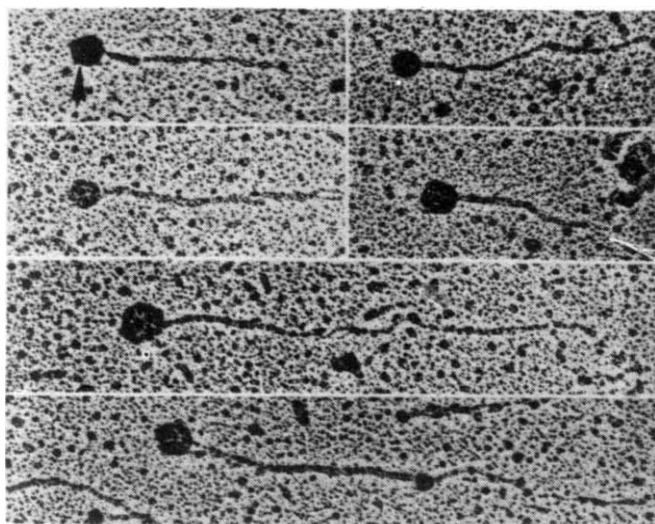


Fig. 10 Electron micrograph of macronuclear DNA complexed with RNA polymerase of *Bacillus subtilis*. Each piece of DNA binds an RNA polymerase molecule at one end.

confirm that all the macronuclear DNA molecules possess a polarity and that all the molecules probably have a property in common at one end. We do not know, of course, whether the binding of RNA polymerase of *B. subtilis* provides an identification of the natural binding site of the *Stylonychia*'s RNA polymerase to macronuclear DNA; RNA polymerase from *B. subtilis* and *E. coli* transcribe macronuclear DNA quite efficiently (unpublished results of D. M. P. and M. R. Lauth).

Finally, the bimodality on the sizes of macronuclear DNA is significant for the following reason. *Stylonychia* synthesizes an RNA precursor molecule with an S value of about 34 in a sucrose gradient. The 34S precursor molecule is processed into a 26S and a 17S molecule of ribosomal RNA. The 34S molecule must have a molecular weight of 2 to 2.5 million. The DNA duplex acting as template for synthesis of the 34S precursor molecule must therefore have a molecular weight of 4 to 5 million. The minor peak of macronuclear DNA at 14S, presumably corresponding to the 2 μm pieces (molecular weight of 4 million) of DNA seen in the electron microscope, has DNA in the right size range to code for ribosomal precursor RNA. Ribosomal RNA of *Stylonychia* has been hybridized with DNA of *E. coli*, with the 10S DNA, and the 14S DNA of the macronucleus. The results (Fig. 11) show a slight amount of binding of the ribosomal RNA to DNA of *E. coli*

and to 10S DNA, but pronounced binding to 14S DNA. The small amount of binding to 10S DNA may represent contamination of 10S DNA with 14S DNA, since the two size classes are difficult to separate cleanly on a sucrose gradient. About 7% of the 14S DNA hybridizes with ribosomal RNA.

The three observations, melting of ends of molecules, binding of RNA polymerase, and presence of the ribosomal genes in the minor 14S peak, support the hypothesis that the small pieces of macronuclear DNA are the functional, transcriptional units of the macronucleus of the living cell.

The average piece of DNA (0.75 μ m) could theoretically code for 750 amino-acids. Since this would imply an unusually large polypeptide, we must assume that less than half of each piece of DNA codes for amino-acids (300 amino-acids would be a more usual size of a polypeptide), or alternatively, many pieces of DNA must code for two or three different polypeptides.

The pieces of macronuclear DNA contain all the genetic information needed to support cell growth and reproduction, as well as all of the metabolic functions of the vegetative cell. This is known because more than 99% of RNA synthesis occurs in the macronucleus during vegetative growth, and the trace of RNA synthesis that occurs in the micronucleus is known to be expendable; amiconucleate strains of *Stylonychia* are easily obtained, and these proliferate for many cell generations without the benefit of micronuclear DNA. Amiconucleated clones do senesce and die sooner, however, than do micronucleated clones.

The observations on *Stylonychia* described here suggest a model of chromosome organization in which a few per cent of the DNA in a band constitutes the genetic locus, and the remaining 93% or so is of unknown significance. The situation is summarized in Fig. 12. The "non-gene" DNA is designated here as "spacer" for lack of information about its possible significance. Each gene locus (or group of gene loci) is separated from the neighbouring gene loci by spacer DNA. During the vesicle stage of the macronuclear anlage transection of the chromosome will occur such that each vesicle contains a gene locus and a stretch of spacer DNA. The arrangement shown in Fig. 12 is similar to the spacer-gene situation present in the DNA containing ribosomal genes. We also note that it has similarities to the structure of the chromosome proposed

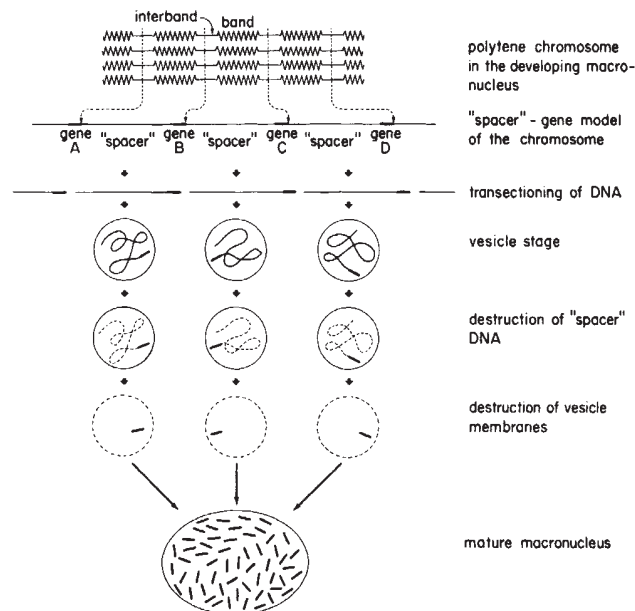


Fig. 12 A scheme to explain the derivation of the gene-sized pieces of DNA in the mature macronucleus from the chromosomes of the micronucleus. The micronucleus goes through several rounds of DNA replication to produce the polytene chromosomes of the early macronuclear anlage. Each band of the polytene chromosomes in the macronuclear anlage is assumed to represent a single genetic locus. The polytene chromosomes are transected between successive bands as shown in Fig. 3. The polytenic copies of DNA in each band become enclosed in a vesicle (Fig. 3). Only one copy is shown in each of the three vesicles in the drawing. Most of the DNA in each vesicle (band) is considered to consist of a spacer that separated the gene copy in one band of a chromosome from the gene copy in the next band. Destruction of the "spacers" occurs in the vesicle stage and is assumed to account for the degradation of 93% of the DNA in the anlage. Only the structural genes with control regions, accounting for 7% of the original chromosomal DNA, are preserved. Destruction of the vesicle membranes produces a macronuclear anlage containing gene-sized pieces of DNA. This DNA is replicated many times to produce the DNA-rich, mature macronucleus.

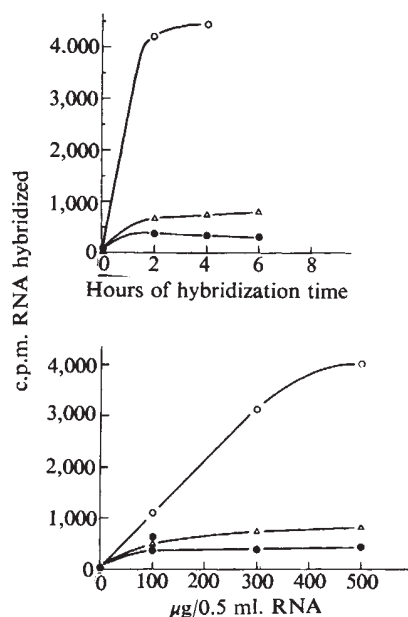


Fig. 11 Hybridization of ribosomal RNA of *Stylonychia* with the 10S (Δ) and 14S ($\circ$) fractions of macronuclear DNA and with DNA of *Escherichia coli* ($\bullet$). The ribosomal RNA complexes slightly with DNA of *E. coli* and slightly more with the 10S DNA of *Stylonychia*. Ribosomal RNA complexes to a much greater extent with the 14S DNA fraction.

by Crick⁸. In the model in Fig. 12 the structural genes are suggested to occupy the interbands, but we have no direct evidence on this point.

The results so far indicate that the macronucleus of *Stylonychia* is a sac of DNA pieces that correspond to little more than single genes for ordinary-sized proteins. Each gene is represented by hundreds of copies. This situation may prove useful in the study of gene structure and function, particularly if various transcription units can be prepared in pure form.

The further study of the steps by which macronuclear DNA is derived from micronuclear DNA may tell us whether the chromosome model in Fig. 12 is correct. We are currently mapping the location of macronuclear DNA sequences within micronuclear DNA as a test of our concept of the chromosome and its processing in *Stylonychia*.

We thank Miss Marlene Lauth for technical assistance. This work was supported by a grant to D. M. P. from the US National Science Foundation.

Received November 1; revised December 8, 1972.

- 1 Ammermann, D., *Chromosoma*, **33**, 209 (1971).
- 2 Ammermann, D., *Arch. Protistenk.*, **108**, 109 (1965).
- 3 Kloetzel, J. A., *J. Cell Biol.*, **47**, 395 (1970).
- 4 Ammermann, D., *J. Cell Biol.*, **40**, 576 (1969).
- 5 Bostock, C. J., and Prescott, D. M., *Proc. US Nat. Acad. Sci.*, **69**, 139 (1972).
- 6 Prescott, D. M., Bostock, C. J., Murti, K. G., Lauth, M. R., and Gamow, E., *Chromosoma*, **34**, 355 (1971).
- 7 Murti, K. G., Prescott, D. M., and Pene, J. J., *J. Mol. Biol.*, **68**, 413 (1972).
- 8 Crick, F., *Nature*, **234**, 25 (1971).

LETTERS TO NATURE

PHYSICAL SCIENCES

Post-Occultation Reception of Lunar Ship America Radio Transmission

THE reception of radio signals from the orbiting lunar spaceship America after its occultation behind the lunar limb is a confirmation of results reported for the Apollo 15 ship Endeavour¹. Similar observations arranged with the lunar command module during the Apollo 16 mission were unsuccessful because transmissions from the command module did not occur while the Moon was above our horizon.

The transmitter used for communication between the lunar command module and the lunar landing party was turned on almost continuously during a period of lunar orbiting between 1755 GMT, December 14, and 0400 GMT, December 15, 1972. The frequency of this transmission was 259.7 MHz with an audio signal at times impressed on the carrier. This transmission was intercepted by a special receiver attached to the 150-foot radio telescope of the US Air Force Cambridge Research Laboratory. This instrument is located at Sagamore Hill near Hamilton, Massachusetts, USA (latitude 42° 37' 55.6" N; longitude 70° 49' 04.5" W).

The spacecraft transmitted during most of the time the Moon was above the local horizon. Post-occultation signals were received when the Moon was near the local zenith. This high lunar altitude facilitated the reception and the signal strengths were much greater than on previous flights.

Occultation of the signal occurred at 0143:36 GMT, December 15, 1972. The signal did not immediately go to a zero level; it seemed to go to zero at approximately 0144:02 GMT and reappeared at 0144:12 GMT, persisted for 52 s, and finally disappeared at 0145:04 GMT. This behaviour indicates a dependence of the post-occultation signal on the exact topography of the lunar surface beneath the spacecraft.

Because the altitude of the Moon was higher than on previous flights, our signal strength was correspondingly greater, and we observed the Fresnel patterns that characteristically accompany occultation. When the signal reappeared, its level was approximately 20 db above noise (as shown by digital voltage recordings). The pen-recorded chart appears in Fig. 1. The AM and FM magnetic tapes and digital sampling of receiver output voltage provide alternative records, and an oscilloscope gave visual and photographic reception.

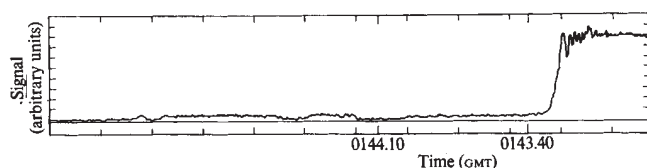


Fig. 1 Record of receiver response (1 division $\equiv$ 10 s).

The height of the America's orbit above the lunar surface was about 96 km at the time of occultation and was reasonably uniform thereafter. The occultation of the Moon ship occurred during 154° of its orbit relative to the lunar centre. During the elapsed time between the occultation and the disappearance

of the signal, the ship traversed about 134 km of the lunar surface. The actual path of the signal was about 6 km less than this figure because the orbit of the ship was inclined to the line of sight to the Earth.

The surface distance traversed between the time of occultation and the time of disappearance of the signal projected on the signal path to the Earth is about 97 km. The effects of libration were insignificant.

Other results from this experiment, such as the possible significance of signals received during the other two occultations, are awaiting further reduction of the data.

Several possibilities for explaining such results have been considered¹⁻³, including refracted waves through the limb of the Moon or lunar surface waves. The immediate continuation of the signal after occultation favours the surface-wave explanation.

We thank the US Air Force Cambridge Research Laboratory for the use of the telescope, Dr Jules Aarons and the operators for assistance, and Dr Edward Lilley and Mr Hays Penfield of Harvard College Observatory for the use of their equipment and for many discussions. We also express our appreciation to NASA and the crew of Apollo 17 for their cooperation.

W. W. SALISBURY
D. L. FERNALD

*Smithsonian Astrophysical Observatory,
Harvard College Observatory,
Cambridge, Massachusetts*

Received January 22, 1973.

¹ Salisbury, W. W., and Fernald, D. L., *Nature*, **234**, 95 (1971).

² Salisbury, W. W., in *Proceedings of the Conference on Electromagnetic Exploration of the Moon* (Ames Research Center, 1970).

³ Salisbury, W. W., *Nature*, **211**, 950 (1966).

Cariaco Trench: Oxidation of Organic Matter and Residence Time of Anoxic Water

THE Cariaco Trench is located on the Caribbean shelf of Venezuela and has a maximum depth of 1,400 m. The surrounding shelf is nowhere deeper than 150 m, thus separating the deep water of the trench from that of the Caribbean. The deep water of the trench is devoid of dissolved oxygen¹. Between the sill depth and 500 m both temperature and salinity decrease, resulting in a slight increase in density which stabilizes the water column somewhat precariously. Below 500 m the water is uniform and well mixed. Such anoxic basins are critically dependent on the balance between supply of organic matter and renewal of the deep water. Although the sediments indicate that at least the deep part of the basin has been permanently anoxic for thousands of years² the relatively small density gradient may facilitate renewal of the deep water on a much shorter time scale. Estimates of the residence time vary between 100 and 2,000 yr (refs. 1,3). Data resulting from a cruise of RV Atlantis II in July 1971 provide a way of assessing the

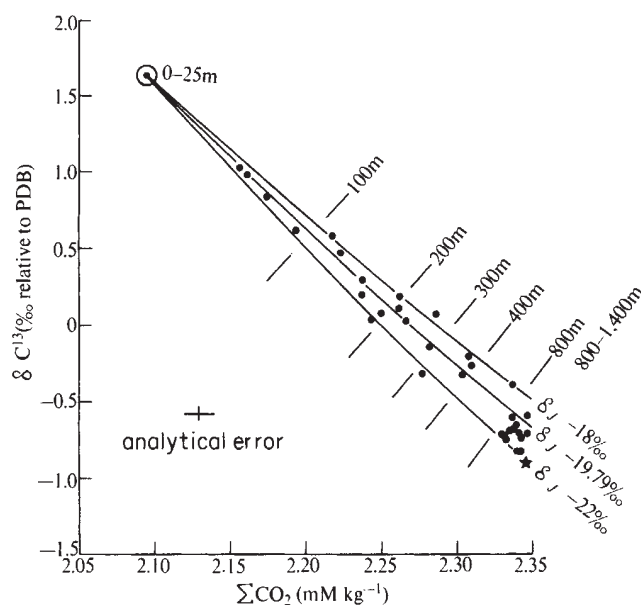


Fig. 1 Amount and $\delta^{13}\text{C}$ of total dissolved inorganic carbon in waters of Cariaco Trench. Curves signify addition of carbon having given $\delta^{13}\text{C}$ (δ_i) to mean surface water. *, Mean deep water in absence of methane formation. Total dissolved inorganic carbon was extracted from the water samples by acidification and gas sparging in a vacuum system¹⁴. After purification and drying, the liberated CO_2 was measured volumetrically and then transferred to a mass spectrometer for isotope analysis. The maximum extent of the analytical error in both kinds of determination is indicated by the error bars in the lower left corner of the figure. The points shown represent samples collected at several stations in the trench. They were superposed in one figure because no significant systematic differences between stations were detected and no single station covered the entire water column in sufficient detail. The uppermost point, labelled 0–25 m, is the mean of individual measurements of six samples from the surface waters; the values were combined in order to eliminate spatial and temporal variations commonly encountered in surface waters. ΣCO_2 is total dissolved inorganic carbon, $\delta^{13}\text{C}$ (‰) = $[(^{13}\text{C}/^{12}\text{C}) \text{ sample} / (^{13}\text{C}/^{12}\text{C}) \text{ standard}] - 1 \times 1,000$, the standard being Pee Dee belemnite (PDB) carbonate.

origin and oxidation of organic matter in the trench and the renewal rate of the deep water.

Measurements of $\delta^{13}\text{C}$ and ΣCO_2 on water samples covering the entire depth range of the Cariaco Trench are shown in Fig. 1. Both $\delta^{13}\text{C}$ and ΣCO_2 show an orderly progression of values from the surface to the deep water of the trench, through the interface between oxygenated and anoxic waters, which was commonly encountered between 250 and 300 m. This progression is the result of the accumulation of oxidized organic carbon in the water column of an anoxic basin⁴. From 800 to 1,400 m no further change can be detected, however, nor for temperature, salinity and other chemical constituents^{1,5}. In Fig. 1, I also show the trajectories for the addition of CO_2 of three different isotopic compositions (δ_i) to the mean surface water. The trajectory $\delta_1 = -19.7\text{‰}$ represents the addition of carbon having the $^{13}\text{C}/^{12}\text{C}$ ratio of the bulk cell carbon of plankton inhabiting the surface water (0–100 m) in the area of

the Cariaco Trench. In spite of the scatter of the data points it seems that, at least to a depth of 400 m, the data fit quite well the addition of oxidized plankton carbon and certainly fall between the two extremes shown. Part of the scatter may be real. There are minor, seemingly random, station-to-station differences; the scatter in the 200 to 300 m interval may be related to the undulating nature of the $\text{O}_2\text{-H}_2\text{S}$ interface or possibly to chemosynthetic carbon fixation near the interface. Much of the irregularity, however, may be caused by analytical error. Data from below 800 m, however, not only cluster very closely but also, with one exception, indicate the addition of carbon of a ^{13}C content lower than of plankton carbon. The difference between mean surface water and mean deep water corresponds to the addition of $0.244 \text{ mmol CO}_2 (\text{kg water}^{-1})$ having a $\delta^{13}\text{C}$ of -21.0‰ . Such carbon is depleted in ^{13}C relative to plankton carbon but is still somewhat enriched relative to the bulk organic carbon contained in surface sediment of the deeper parts of the trench. For this material $\delta^{13}\text{C}$ is $\sim -22\text{‰}$.

Several processes may cause deviations in the deep water from the trajectory described by addition of oxidized plankton carbon to the ΣCO_2 . Because measurements of ΣCO_2 and $\delta^{13}\text{C}$ can reveal only the sum of all possible effects, I cannot rule out *a priori* any processes which may result in changes different from those observed because they may have been masked or cancelled by others. The principal processes to be considered are carbonate solution or precipitation, methane formation, fractionation during oxidation, and introduction of organic matter derived from the continent. Chemosynthetic fixation is not likely to be significant at this depth.

Part of the observed deviation could be produced by carbonate precipitation in the deep anoxic waters which would result in the removal of CO_2 enriched in ^{13}C from the water; but the sediments show no evidence for this. Nor is there any indication of carbonate solution which would result in the addition of heavier carbon to the ΣCO_2 . (Bulk carbonate in the surface sediment has a $\delta^{13}\text{C}$ of $+1.2\text{‰}$). V. J. Linnenbom and J. W. Swinnerton (unpublished) measured methane concentrations between 0.1 and 0.15 ml. l^{-1} below 800 m in the Cariaco Trench. Because bacterial methane formation utilizes principally CO_2 (ref. 6) and the methane formed is strongly depleted in ^{13}C relative to the source material⁷, this process will influence both ΣCO_2 and $\delta^{13}\text{C}$ in deep water. If the methane formed is isotopically similar to that formed in the Black Sea ($-65 (\pm 5)\text{‰}$, ref. 8) one can calculate the approximate effect of methane formation on ΣCO_2 and $\delta^{13}\text{C}$. For organic carbon oxidized in the deep water $\delta^{13}\text{C} \approx -22\text{‰}$, identical to the organic carbon in the surface sediment. This agreement between the ^{13}C contents of oxidized and sedimentary carbon suggests that there is no significant overall isotope fractionation during anaerobic oxidation. It is possible, however, that anaerobic oxidation preferentially affects a different set of organic components from those affected by aerobic oxidation and that this difference is at least partly responsible for the difference between the isotopic composition of the CO_2 added in the near-surface water and that added in the anoxic deep water.

The low $\delta^{13}\text{C}$ values of the sedimentary organic matter and of the organic carbon oxidized anaerobically can also be

Table 1 Range of Quantities Entering Calculation of Residence Time of Deep Water in Cariaco Trench

Quantity	Minimum	Maximum	Units	Remarks
Excess CO_2 in deep water	0.07	0.11	mmol kg^{-1}	Below interface and sill depth
Total oxidized organic C	840	1,320	g m^{-2}	Integrated over 1,000 m
Primary production	90	250	$\text{g C m}^{-2} \text{ yr}^{-1}$	Refs. 1, 12, 13
Flux to deep water	4.5	50	$\text{g C m}^{-2} \text{ yr}^{-1}$	5% and 20% of primary production ¹¹
Addition from continent	0	25	$\text{g C m}^{-2} \text{ yr}^{-1}$	0 and half of flux to deep water (^{13}C data)
Oxidation in deep water	2.3	38	$\text{g C m}^{-2} \text{ yr}^{-1}$	Half of total input to deep water ¹¹
Residence time	22	570	yr	Geometric mean: 112 yr

explained as being due to a contribution of continental organic matter to the sediment in the Cariaco Trench. Land plants are depleted in ^{13}C relative to marine plants⁹ and the location of the trench is such that a contribution of sediment from land is to be expected. One of the cores taken on the recent cruise penetrated a tree trunk, 24 cm in diameter and buried just over 0.5 m; wood from the centre of the trunk had a ^{14}C age of $5,400 \pm 100$ yr and $\delta^{13}\text{C}$ of -26.9% . Assuming a $\delta^{13}\text{C}$ of -26 to -27% for organic matter from land¹⁰, I calculate that the continental contribution to the sedimentary organic matter in the trench may be as high as one-third, while the marine contribution is two-thirds or more.

The amount of oxidized organic carbon which has accumulated in the deep water is $0.07 \text{ mmol kg}^{-1}$ relative to the water at the $\text{O}_2\text{-H}_2\text{S}$ interface and $0.11 \text{ mmol kg}^{-1}$ relative to sill depth (Fig. 1). To estimate the residence time of the deep water one needs, in principle, only to divide the total accumulation by the oxidation rate. In practice, the oxidation rate cannot be assessed very precisely. But by using available figures of the primary productivity of the area and drawing some analogies to the situation in the Black Sea, I have set limits on the oxidation rate which are unlikely to be exceeded (Table 1). Both extremes for the residence time should be regarded as highly unlikely because they incorporate successive minimization and maximization, respectively, at each step in the calculation. The likeliest value should lie near the geometric mean of the extremes, ~ 100 yr.

I thank officers and crew of Atlantis II for help and E. Ross for assistance, supported by the Office of Naval Research.

W. G. DEUSER

Woods Hole Oceanographic Institution,
Woods Hole, Massachusetts 02543

Received January 22, 1973.

- ¹ Richards, F. A., and Vaccaro, R. F., *Deep-Sea Res.*, **3**, 214 (1956).
- ² Heezen, B. C., Menzies, R. J., Broecker, W. S., and Ewing, M., *Geol. Soc. Amer. Bull.*, **69**, 1579 (1958).
- ³ Redfield, A. C., Ketchum, B. H., and Richards, F. A., in *The Sea* (edit. by Hill, M. N.), **2**, 26 (Interscience, London, 1963).
- ⁴ Deuser, W. G., *Science*, **168**, 1575 (1970).
- ⁵ Okuda, T., Benítez, J., and Fernández, E. A., *Bol. Inst. Oceanogr. Univ. Oriente*, **8**, 28 (1969).
- ⁶ Wolfe, R. S., *Adv. Microbial Physiol.*, **6**, 107 (1971).
- ⁷ Rosenfeld, W. D., and Silverman, S. R., *Science*, **130**, 1658 (1959).
- ⁸ Hunt, J. M., *Amer. Assoc. Petrol. Geol. Mem.* (in the press).
- ⁹ Wickman, F. E., *Geochim. Cosmochim. Acta*, **2**, 243 (1952).
- ¹⁰ Craig, H., *Geochim. Cosmochim. Acta*, **3**, 53 (1953).
- ¹¹ Deuser, W. G., *Deep-Sea Res.*, **18**, 995 (1971).
- ¹² Curl, jun. H., *Deep-Sea Res.*, **7**, 183 (1960).
- ¹³ Ballester, A., *Mem. Soc. Cienc. Nat. La Salle*, **29**, 122 (1969).
- ¹⁴ Deuser, W. G., and Hunt, J. M., *Deep-Sea Res.*, **16**, 221 (1969).

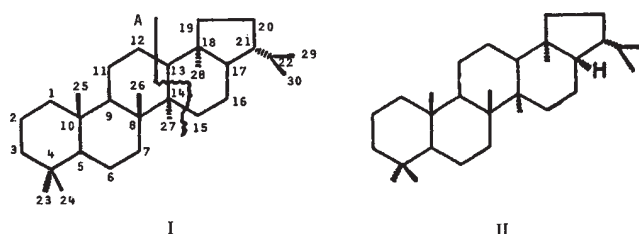
17 α (H) Hopane identified in Oil Shale of the Green River Formation (Eocene) by Carbon-13 NMR

THE occurrence of steroid and terpenoid saturated hydrocarbons in organic sediments is well known and their geological significance has been established¹⁻⁴. These steranes and terpanes represent the complete reduction of naturally occurring steroids or terpenoids to relatively stable fossil molecules which preserve intact the complete molecular skeleton and stereochemistry of their precursors. Steranes and terpanes, together with other "biological markers", provided convincing proof for the biological origin of petroleum, oil shales, and other organic matter in sediments all over the world.

During the investigation of ^{13}C NMR shifts and the structural correspondence of pentacyclic triterpenes we undertook a ^{13}C NMR study on one of the most abundant components of the hexane soluble fraction of oil shale bitumen of the Green River Formation. Gelpi *et al.* isolated and collected this compound

for the first time employing their fully automatic capillary preparative gas chromatography system⁵.

The compound is probably identical with the one representing GC peak No. 20 in the chromatogram described by Henderson *et al.*⁶, and possibly the same constituent was detected by Hills *et al.*⁷ as gas liquid chromatography (GLC) peak G in Nigerian crude oil distillates. Its molecular composition was $\text{C}_{30}\text{H}_{52}$, and it was tentatively identified on the basis of combined gas chromatography/mass spectrometry (GC/MS) data as a pentacyclic triterpane, possibly belonging to the hopane family⁸. Comparative studies with authentic standards employing capillary column GLC and MS suggest that this compound is 17 α (H) hopane, structure II^{8,9}. Here we present a rigorous proof derived exclusively from ^{13}C NMR data for the structure and stereochemistry of that important triterpenoid fossil molecule.



Using the preparative GLC technique and experimental conditions of Gelpi *et al.* (see Fig. 6D in ref. 5; thiourea non-adduct fraction) we collected 18.4 mg of white crystalline material of 97% purity (GC estimate). The substance melts at 155.5°C and is optically active; $[\alpha]_{\text{D}}^{20}$ is $+59.5$ in CHCl_3 .

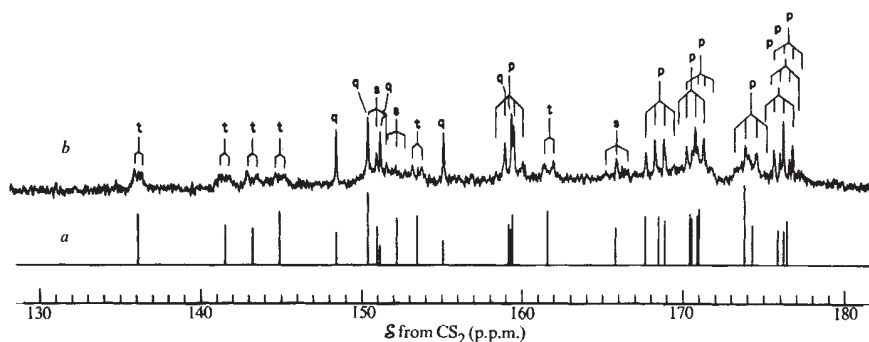
The ^{13}C NMR spectra were obtained on a Varian XL-100-15 CW spectrometer and decoupler, modified for Fourier transform spectroscopy with appropriate gating. A 1,000 W pulsed r.f. transmitter (Lawrence Berkeley Laboratory), 4-pole Butterworth filter, 15-bit analogue/digital converter, home-built computer interface and experimental controller, and 16 kwords of dedicated (32-bit word size) core memory, accessible to the rest of the 80 kwords of an XDS Sigma 7 computer for fast Fourier transform and 'CalComp' plot, complete the principal components of the system.

With the 16 k memory, the typical "computer resolution" for the normal range of ^{13}C chemical shifts has been 0.6 Hz or 0.025 p.p.m. entirely satisfactory for the intrinsic line width of compounds of polycyclic type, which is no better than 0.5 to 1.0 Hz under the experimental conditions (solvent, temperature, viscosity and so on). Also, for long signal averaging, the 32-bit word size avoids "wrap around" problems normally faced by computers with 16 to 20-bit word size. Chemical shift axes are plotted simultaneously with the spectra, giving high precision in shift measurements relative to an internal standard. Other subroutines have been developed to integrate line intensities, determine line widths, and perform other functions. Pulse decoupling¹⁰ to avoid nuclear Overhauser intensity errors in intensity measurements has also been incorporated. A detailed discussion of operation and performance of on-line instrumentation will be published elsewhere.

The ^{13}C spectra of the collected unknown in both the proton noise decoupled mode and in the coherent off-resonance decoupled mode are shown in Fig. 1. As a rule, not one but several spectra were made in the off-resonance mode^{11,12} to obtain identification from the splitting patterns caused by incomplete proton decoupling.

The line diagram (Fig. 1a) shows thirty ^{13}C resonances corresponding to thirty carbon atoms in the molecule. The chemical shift range of the resonances indicates complete saturation of all carbons present. The off-resonance spectra (Fig. 1b) revealed the chemical nature of these carbons—eight methyl groups and six methine, eleven methylene and five

Fig. 1 The natural abundance ^{13}C NMR spectra of the triterpane isolated from the bitumen of the oil shale of Green River Formation. *a*, Computer printout of the line diagram of the noise-decoupled spectrum. Resonance intensities are indicated by the height of the lines. *b*, Off-resonance spectrum. p, Primary; s, secondary; t, tertiary; q, quaternary carbons.



quaternary carbons were identified by examination of the splitting patterns. These carbons account for a molecular composition of $\text{C}_{30}\text{H}_{52}$ and suggest a pentacyclic triterpane. The presence of a five-membered ring is depicted by the number of quaternary carbon resonances (five) together with a tertiary resonance at 161.6 p.p.m. assigned to a methine carbon of an isopropyl group.

Comparisons within a series of pentacyclic triterpanes employing authentic hopane, moretane, lupane, and oleanane (B. B., P. C., D. M. W., and A. L. B., to be published) afforded further structural details. Two criteria were used in the comparison: (a) chemical shift agreement within 0.2 p.p.m.; (b) chemical identity of the resonances as derived from off-resonance data. It was found that the spectra of all five compounds contain an identical resonance pattern consisting of eighteen resonance lines, corresponding to eighteen identical carbon atoms. Considering the principle of structural additivity¹³, it was concluded that the identical resonance pattern must represent an identical partial structure present in the analogues as well as in the unknown; for example the portion A in the structure of hopane (I). Detailed comparison with

hopane (I) which served henceforth as a "zero chemical shift" standard led to the completion of our structure elucidation (Fig. 2).

The close agreement of the quaternary resonance patterns (marked by asterisks) in both spectra suggests stereoisomeric relations between the two compounds, because methyl substitutions or skeletal changes usually result in far greater chemical shift distortions of bridgehead carbons in triterpenes.

The chemical shift pattern of the tertiary carbons needed detailed examinations. Seemingly, only one tertiary on C(17) has experienced a chemical shift of 15 p.p.m. upfield. This is a rather large distortion to be caused on one atom by steric interactions only, and the large shift probably represents a composite shift with the involvement of several tertiaries—C(17), C(13), and C(21)—but, by accidentally replacing each other, they lead to the degeneracy of the number of the observable chemical shifts. Owing to the close identity of the quaternary carbon patterns, the shifts of the tertiary carbons do have to stem from steric perturbations associated chiefly with through-space, possibly " γ -gauche", interactions¹⁴.

Examination of methyl group positions quickly establishes

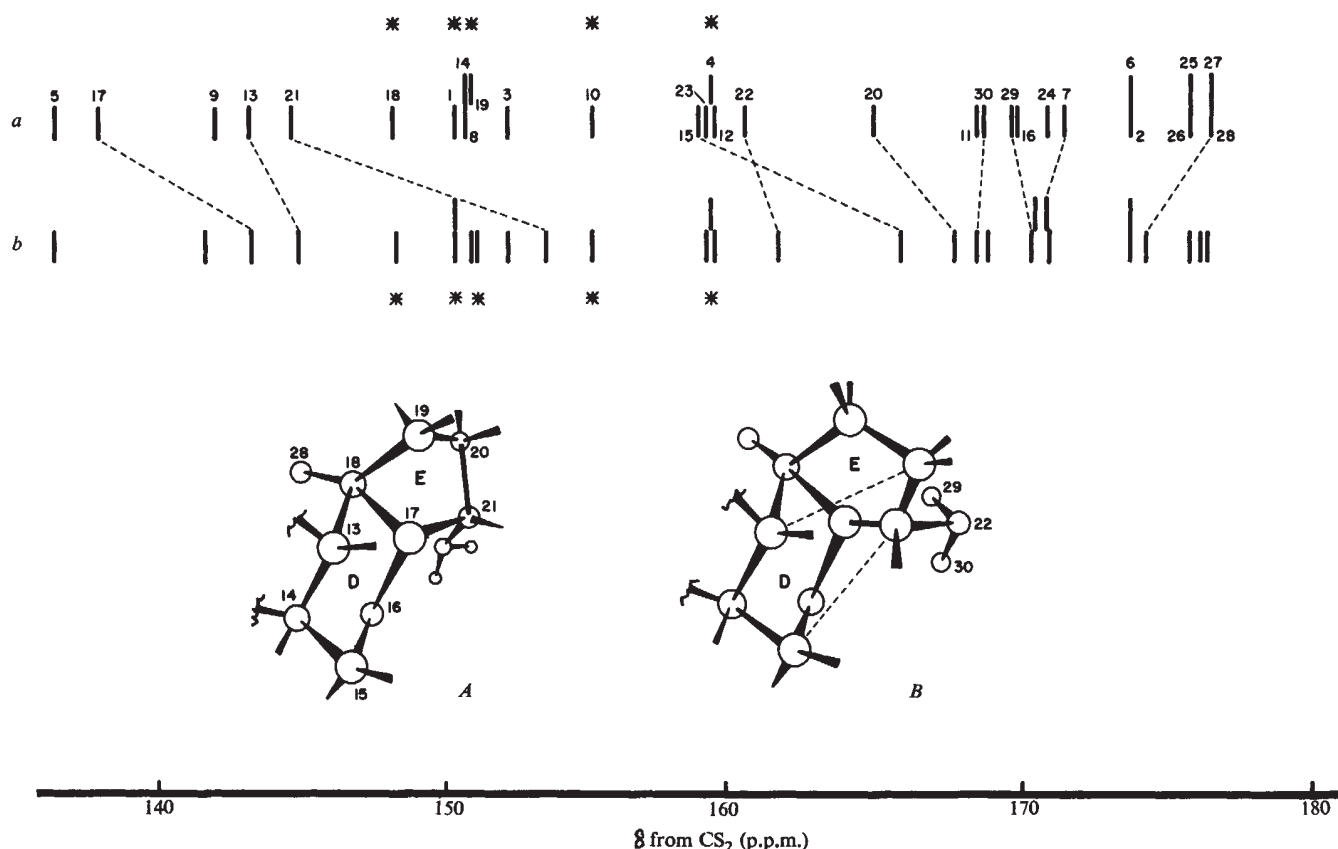


Fig. 2 ^{13}C chemical shift line diagram of (a) hopane, (b) unknown. Asterisks denote quaternary carbons; Dreiding models of D-E rings of (A) hopane and (B) 17 α (H) hopane respectively.

that the role of axial methyls in these interactions is negligible. Methyls at C(23), (24), (25), (26), and (27) were not shifted at all; shifts involving the isopropyl group were readily assigned, and only C(28) exhibited a downfield shift of only 2.2 p.p.m. This leaves methylene-methine through-space interactions to account for the observed shifts of the tertiary carbons and, from the foregoing considerations, it is certain that the involved methylenes must be located in either D or E rings.

Such interactions can then only be affected by the inversion of the D-E ring junction which is *trans* in hopane (I) into a *cis* configuration resulting in several new γ -*gauche* interactions between tertiary and secondary ring carbons in 17 α (H) hopane (II). Adapting structure II the shifts have been uniquely assigned as follows: C(21) is shifted 8.8 p.p.m. upfield because of two new buttressed γ -*gauche* involvements with C(15) and C(13); C(20) is shifted 2.6 p.p.m. associated with C(13); C(17) is shifted 5.2 p.p.m. upfield owing to its involvement in increased numbers of vicinal-*gauche* configurations. By *cis*-inversion of C(17), the C(28) methyl is bent away from D-ring carbons and the decreased steric perturbation resulted in a slight downfield shift (Fig. 2). This elucidation fully accounts for the variants of resonance pattern between the isolated triterpane and the selected analogue and is in rigorous accordance with the established rules of ^{13}C NMR chemical shift. Further examination proved that none of the other twenty-four theoretically possible stereoisomers would correspond to the observed data. Thus, using the techniques of ^{13}C NMR we have conclusively established the structure of the isolated triterpane to be 17 α (H) hopane.

Table 1 ^{13}C Chemical Shifts in p.p.m. from CS_2 and Assignments for the Spectra of Hopane and 17 α (H) Hopane

Carbon	Hopane	17 α (H) hopane
1	150.4	150.4
2	173.8	173.8
3	152.2	152.2
4	159.4	159.2
5	136.4	136.2
6	173.8	173.8
7	171.6	170.8
8	150.6	150.4
9	142.0	141.5
10	155.2	155.1
11	168.6	168.8
12	159.5	159.4
13	143.2	144.8
14	150.8	151.1
15	158.9	165.8
16	169.9	170.5
17	137.9	143.2
18	148.2	148.2
19	150.9	150.9
20	164.9	167.7
21	144.6	153.4
22	160.5	161.6
23	159.2	159.1
24	171.0	171.0
25	176.0	175.9
26	175.9	176.2
27	166.7	176.4
28	176.8	174.3
29	169.7	170.4
30	168.7	168.6

^{13}C chemical shifts and assignments for the spectra of hopane and 17 α (H) hopane are compiled in Table 1.

We thank Miss Patricia Wszolek and Dr Emilio Gelpi for the isolation of the triterpane. This work was supported by the National Aeronautics and Space Administration.

B. BALOGH
D. M. WILSON
P. CHRISTIANSEN
A. L. BURLINGAME

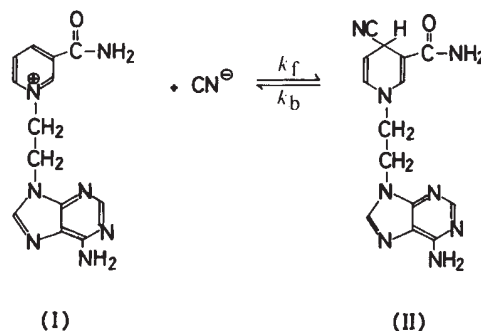
Space Sciences Laboratory,
University of California,
Berkeley, California 94720

Received October 24, 1972; revised February 9, 1973.

- Meinschein, W. G., *Space Sci. Rev.*, **2**, 653 (1963).
- Bradley, W. H., *Geol. Soc. Amer. Bull.*, **77**, 1333 (1966).
- Henderson, W., Wollrab, V., and Eglinton, G., in *Advances in Organic Geochemistry* (edit. by Schenk, P. A., and Havenaar, I.), 181 (Pergamon Press, New York, 1968).
- Anderson, P. C., Gardner, P. M., Whitehead, E. V., Anders, D. E., and Robinson, W. E., *Geochim. Cosmochim. Acta*, **33**, 1304 (1969).
- Gelpi, E., Wszolek, P. C., Yang, E., and Burlingame, A. L., *Anal. Chem.*, **43**, 864 (1971).
- Henderson, W., Wollrab, V., and Eglinton, G., *Chem. Commun.*, 710 (1968).
- Hills, I. R., and Whitehead, E. V., *Nature*, **209**, 977 (1966).
- Whitehead, E. V., *Chem. Ind.*, 1116 (1971).
- Whitehead, E. V., *Inter. Symp. Hydrogeochemistry and Biogeochemistry, Tokyo, Abst. Pap., Sci. Coun. Jap.-Inter. Ass. Geochem. Cosmochem.*, 80 (Tokyo, 1970).
- Freeman, R., Hill, H. D. W., and Kaptein, R., *J. Magn. Res.*, **7**, 327 (1972).
- Birdsall, B., Birdsall, N. J. M., and Feeney, J., *Chem. Commun.*, 316 (1972).
- Packler, K. G. R., *J. Magn. Res.*, **7**, 442 (1972).
- Balogh, B., Wilson, D. M., and Burlingame, A. L., *Nature*, **233**, 261 (1971).
- Dalling, D. K., and Grant, D. M., *J. Amer. Chem. Soc.*, **94**, 5318 (1972).

Can Polyelectrolytes be called Catalysts?

It is now well established that interionic reactions can be extensively enhanced^{1,2} or retarded^{3,4} by addition of polyelectrolytes, but whether the forward or reverse reaction is affected by polyelectrolytes is unknown. Here we report a kinetic investigation of an addition reaction of CN^- to a nicotinamide derivative — N-2'-(9-ethyladeninyl)-3-carbamoyl pyridinium chloride (NAA) (I), synthesized in this laboratory⁵ in the presence and absence of polyelectrolyte. A similar reaction of nicotinamides quaternized with alkyl groups was also thoroughly investigated^{6,7}.



The second-order rate constant of the forward reaction k_f was determined by the stopped-flow technique using a Hitachi spectrophotometer model RSP-2. The temperature was 25°C. The appearance of II was followed at 340 nm. The equilibrium constant $K (=k_f/k_b)$ was obtained from the equilibrium optical density using the method of Cordes⁸.

As in the ammonium cyanate-urea conversion⁴, a reaction between unlike charged ionic species, reaction (I) was retarded by the addition of sodium polystyrenesulphonate (NaPSt). In other words the ratio k_f/k_o (k_o is the rate constant without polyelectrolyte) was smaller than unity. Furthermore the effect of the polyelectrolyte on the equilibrium constant, K/K_o (where K_o is the equilibrium constant without polyelectrolyte), was also smaller than unity. At an NaPSt concentration of 2 mE l.⁻¹ both k_f/k_o and K/K_o were 0.5. These ratios decreased with increasing concentration in the same proportion.

The effect of polyelectrolyte on the elementary processes can be discussed in terms of the free energies (Table 1). By polyelectrolyte addition $\Delta G^\ddagger$ and ΔG were increased, which

Table 1 Thermodynamic Data of the NAA-CN⁻ Reaction with and without Polystyrenesulphonate (25° C)

[NaPSt] (E l. ⁻¹)	$\Delta G^\ddagger$ (kcalorie mol ⁻¹)	ΔG (kcalorie mol ⁻¹)	$\Delta G^\ddagger - \Delta G$ (kcalorie mol ⁻¹)
0	17.5	-3.1	20.6
2×10^{-3}	18.0	-2.6	20.6

$\Delta G^\ddagger$, standard free energy of activation; ΔG , free energy of reaction.

is in agreement with the observed changes (mentioned above) of k_f and K . It should be mentioned that the value of ($\Delta G^\ddagger - \Delta G$) was 20.6 kcalories mol⁻¹ with and without the polyelectrolyte. The same value was also found for sodium polyethylene sulphonate and a diethyldiallylammonium chloride-SO₂ copolymer. These results show that the forward reaction was retarded whereas the backward reaction was not affected at all, and it was the initial state (neither the activated nor the final state) that was influenced by polyelectrolyte addition, in the reaction under consideration.

Our work suggests that polyelectrolytes should not be regarded as true catalysts according to the definition of Ostwald⁹, who claimed that a catalyst must influence the forward and reverse reaction rates in the same proportion. Thus the term "polyelectrolyte catalysis" (or "polymer catalysis" in general), which is becoming popular, should be avoided.

Centeno *et al.*¹⁰ have observed that addition of cyanide ion to N-hexadecyl 3-carbamoyl pyridinium ion was enhanced by a factor of 10^3 by hexadecyltrimethyl ammonium bromides and the association equilibrium constant was increased by a factor of about 2.5×10^4 . This means that the backward reaction was retarded by a factor of 25. This observation apparently contradicts our results, but can be explained in terms of the hydrophobic interaction which is much greater than the repulsive forces between the positive charges of the surfactant and the substrate. Although the reason for the deceleration of the reverse reaction is not clear, we note that the forward and reverse reactions were influenced at different rates. Thus micelles are not catalysts either.

This work was supported by the Japanese Ministry of Education.

Note added in proof. It should be mentioned that the micellar influence on rate and equilibrium constant was reported earlier by Cordes *et al.*¹¹ and Fendler *et al.*^{12,13}. In these studies, the rate and the equilibrium constant were shown to be affected in a different proportion, as was the case for systems mentioned in the text.

NORIO ISE
TSUNEO OKUBO

Department of Polymer Chemistry,
Kyoto University,
Kyoto

Received December 4, 1972.

- Morawetz, H., *Acc. Chem. Res.*, **3**, 354 (1970).
- Ise, N., *Adv. Polymer Sci.*, **7**, 536 (1970-1971).
- Morawetz, H., and Shafer, J. A., *J. Phys. Chem.*, **67**, 1293 (1963).
- Okubo, T., and Ise, N., *Proc. Roy. Soc.*, **A327**, 413 (1972).
- Okubo, T., and Ise, N., *Tetrahedron Lett.* (in the press).
- Wallenfels, K., and Diekmann, D., *Ann. Chem.*, **621**, 166 (1959).
- Lindquist, R. N., and Cordes, E. H., *J. Amer. Chem. Soc.*, **90**, 1269 (1968).
- Behme, M. T. A., and Cordes, E. H., *Biochim. Biophys. Acta*, **108**, 312 (1965).
- Ostwald, W., *Phys. Z.*, **3**, 313 (1902).
- Centeno, M., Lehrmann, G., Urdaneta, M., Lindquist, P., Dunham, D., Price, M., Sears, B., and Cordes, E. H., *J. Amer. Chem. Soc.*, **94**, 8164 (1972).
- Baumrucker, J., Colzadilla, M., Centeno, M., Lehrmann, G., Lindquist, P., Dunham, D., Price, M., Sears, B., and Cordes, E. H., *J. Phys. Chem.*, **74**, 1153 (1970).
- Fendler, E. J., and Fendler, J. H., *Chem. Commun.*, 816 (1970).
- Casilo, L. M., Fendler, E. J., and Fendler, J. H., *J. Chem. Soc.*, **B**, 1377 (1971).

Second Law of Thermodynamics

McCLARE¹ has proposed a complicated new statement of the Second Law of Thermodynamics. I wish to propose the following simple one:

"The only processes that can happen spontaneously are those that can in principle be made to yield work, for example, to lift up a weight."

Although I know of nearly a dozen statements of this Law which distinguish with varying degrees of clarity and universality those processes that can happen spontaneously from those that cannot, I have not come across the statement proposed except in the restricted field of mechanics: perhaps something similar is implied in a footnote by Everett².

A few examples of natural spontaneous processes will illustrate what is intended. Heat can flow only from a higher temperature to a lower, and if the flow takes place through a heat engine (for example, a thermojunction) part of the heat is converted into work. Diffusion of solutes occurs only from a high concentration to a lower one and this may be made to yield work by an arrangement of selective membranes (a van't Hoff box): should the solutes be electrically charged the arrangement can yield electrical work; this occurs during an action potential. Finally, it is well known that a chemical system subject to constant external pressure and under isothermal conditions, with negligible temperature gradients within itself, and at the same temperature T_e as its environment, will tend to react until its Gibbs's free energy is a minimum. This means that the reaction tends to continue spontaneously while the system is capable of performing work (by muscle, galvanic cell or van't Hoff box) and the reaction stops when it can no longer yield work.

A wide variety of examples could be given, and I have not come across any contrary ones: but it could be argued that it is impossible to prove a general case from particular examples, no matter how numerous. The general proof that follows is based on the fact that the creation of entropy is related in a simple fashion to the capacity for performing work.

One of the best, because most general, of the conventional statements of the Second Law is that the only processes that can happen spontaneously are those in which the entropy of the Universe is increased. The amount of entropy created, ΔS_{cr} , is thus positive for spontaneous processes and approaches zero as reversible equilibrium is approached. It can be shown that for any change of thermodynamic state, including changes of temperature, of a non-isolated system in an environment at temperature T_e there corresponds a certain maximum work performance, w_{max} , where $w_{max} = -\Delta U + T_e \Delta S$. If the work actually obtained is w it can be shown that the entropy created is

$$\Delta S_{cr} = (w_{max} - w)/T_e$$

Clearly w must take a value between zero and w_{max} ; accordingly ΔS_{cr} must always be positive and it will have its greatest value when no external work is done. This argument can be easily extended to an isolated system: in that case the work resulting from the spontaneous change must be stored as such within the system. Thus the accepted criterion for spontaneity, that $\Delta S_{cr} > 0$, is the same as the proposed criterion that the system be capable of performing work.

Is anything gained by yet another addition to the anthology of statements of the Second Law? I believe that it is illuminating to see that the arcane world of thermodynamics is so similar to the everyday world of mechanics: and I think that many scientists find the concept of work simpler to understand and to manipulate correctly than the concept of entropy. For biologists in particular, who are continually concerned with systems that actually perform large amounts of mechanical electrical and osmotic work, I hope that my proposal represents a clarification.

I am not convinced that McClare really needs to put forward a new statement of the Second Law to support his principal

argument¹ that work corresponding to the free energy change of ATP splitting must somehow be stored as work if it is later to produce mechanical effect, and that if it becomes converted into heat it will for this purpose be lost. The Kelvin statement of the Second Law suffices to support this argument by showing that it is impossible to convert heat back into work in an isothermal system. I am not competent to judge the feasibility of McClare's detailed proposals for the mechanism of this work storage but would point to the increasing experimental evidence⁴ that work is somehow stored in the crossbridges of muscle for many seconds, perhaps much longer; if his proposals will not do, others must be found. Finally, I would plead for more thought to be given to the problems that arise when the traditionally macroscopic arguments of thermodynamics must be applied to very small systems which, as McClare says, "use . . . one molecule of ATP at a time". It is far from clear³ whether we must modify our basic ideas or not.

D. R. WILKIE

Department of Physiology,
University College London,
Gower Street, London WC1E 6BT

Received January 22, 1973.

¹ McClare, C. W. F., *Nature*, **240**, 88 (1972).

² Everett, D. H., *Chemical Thermodynamics*, 27 (Longman, London, 1971).

³ Hill, T. H., *Thermodynamics of Small Systems* (Benjamin, New York, 1963).

⁴ Bagshaw, C. R., and Trentham, D. R., *Biochem. J.* (in the press).

BIOLOGICAL SCIENCES

Origin of the Dutch Elm Disease Epidemic in Britain

WE have shown that two culturally distinguishable groups of *Ceratocystis ulmi* are present in Britain, one "fluffy" and aggressive, the other "waxy" and non-aggressive, and have presented evidence that the former is responsible for the current epidemic of Dutch elm disease¹. It is of obvious interest to consider the origin of the aggressive strain.

There seem to be two chief alternatives: either that the aggressive strain arose by genetic change within an endemic non-aggressive gene pool, or that it was introduced to Britain. The separation of *C. ulmi* into two groups, apparently without a range of intermediates¹, can be reconciled with either alternative if we assume a rapid clonal build-up of the aggressive strain within the *C. ulmi* population once it had appeared.



Fig. 1 Sample of bark from Rock elm log RR 11, showing breeding galleries of *H. rufipes*.

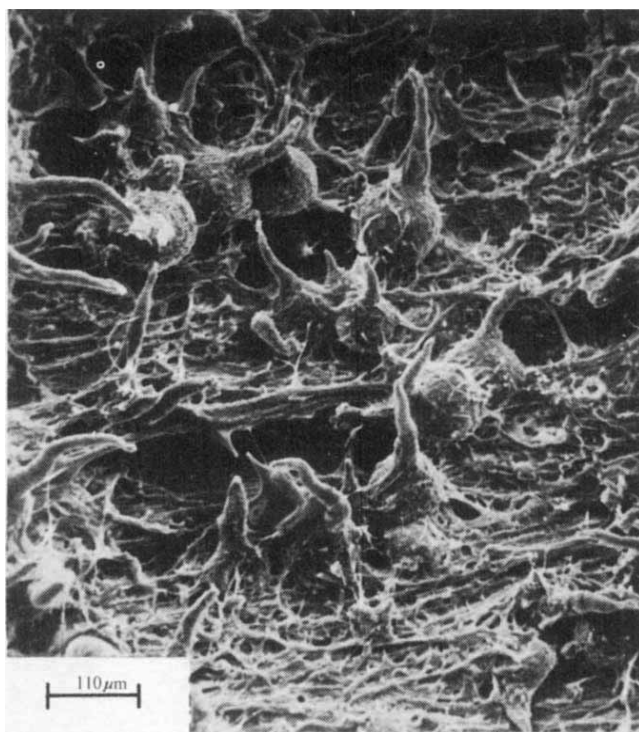


Fig. 2 Stereoscan photomicrograph of breeding gallery of *H. rufipes* in the bark of RR 11, showing mature and developing perithecia of *C. ulmi* surrounded by mycelium, and spore slime from nearby coremia.

The close morphological and pathological similarity between the British aggressive and some North American isolates¹ suggests introduction, but can be reconciled with the theory of genetic change if a functional relationship exists between fluffy culture-type and aggressiveness.

Data from disease distribution are also inconclusive. Early Forestry Commission records indicate that the epidemic became evident in simultaneous outbreaks of severe disease in two widely separated areas, North Gloucestershire and South Essex, between 1965 and 1967. Although a similar genetic change in both areas during the same period seems unlikely, the records are too imprecise to preclude the possibility of material from a single disease origin being transported to the other. The introduction alternative seems more plausible for such widely spaced outbreaks of disease. These considerations have now been supplemented by strong evidence in favour of introduction.

In January 1973, we examined at Southampton Docks two 40 foot containers of Rock elm logs (*Ulmus thomasii*), recently imported from Toronto. The logs, up to about 35 foot long and 2 foot in diameter, were fresh and with the bark attached. Examination revealed that of the twenty logs, seven showed staining of doubtful cause in the bark and wood, and two others

Table 1 Comparison of Growth Rates of Waxy, Fluffy Aggressive and Rock Elm Isolates of *C. ulmi*

Moreton waxy isolates	Growth rate (mm day ⁻¹)				Rock elm isolates
	Tewkesbury	Fluffy isolates Essex	Chichester		
(M3) 2.65	(T14) 3.90	(O26) 3.78	(C19) 4.13	(RR 2)	3.78
(M5) 2.35	(T25) 3.68	(O29) 3.48	(C33) 3.40	(RR 7)	3.70
(M6) 2.40	(T39) 3.83	(O39) 3.58	(C37) 3.78	(RR 10)	3.60
(M8) 2.40	(T44) 3.55	(O42) 3.65	(C46) 3.70	(RR 11)	3.65
Mean 2.45	3.74	3.62	3.75		3.68

Isolate numbers in parentheses. Data for individual isolates are means of two replicates after 5 days growth on 20 ml. of 2% 'Oxoid Malt Extract Agar' at 18° C.

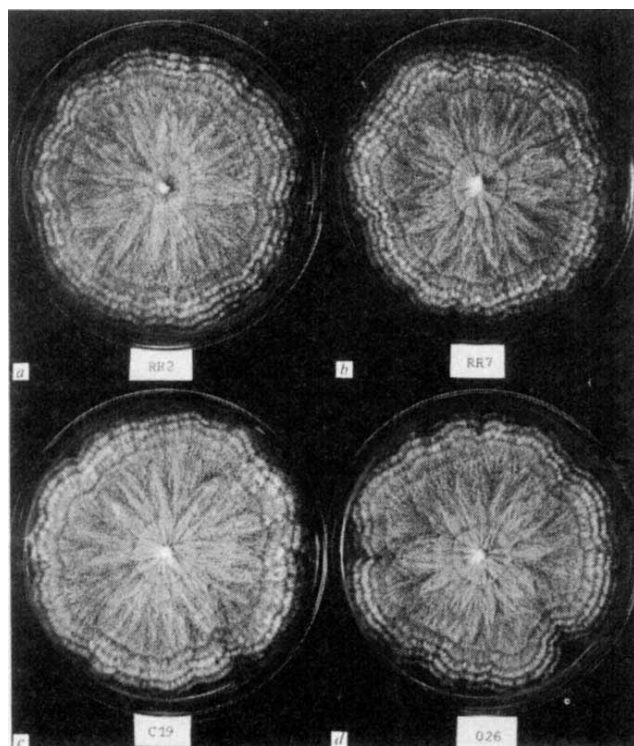


Fig. 3 Comparison of cultures of Rock elm and British aggressive isolates of *C. ulmi* on 2% 'Oxoid Malt Extract Agar' after 1 week in darkness at 18° C, followed by exposure to light. a, b, Rock elm isolates RR 2 and RR 7 respectively; c, d, British aggressive isolates C19 and O26 respectively. (See also Table 1.)

(RR 10 and RR 11) showed the distinct streaking in the outermost xylem vessels characteristic of Dutch elm disease. Samples were taken from four logs, including RR 10 and RR 11, and from each *C. ulmi* was isolated. In RR 10 and RR 11 extensive colonization of the inner bark by *C. ulmi* had occurred, and swards of coremia (asexual fruiting bodies) and mycelium were found in fissures between the inner and outer bark. In the bark, breeding galleries (Fig. 1), live adults, eggs and larvae of *Hylurgopinus rufipes*, a North American vector of Dutch elm disease, were found in quantity. The breeding galleries in one

log (RR 10) were packed with sticky coremial spore masses of *C. ulmi*. In the breeding galleries of RR 11 we found not only coremia but also abundant perithecia, the sexual stage of *C. ulmi* (Fig. 2). Many perithecia were exuding ascospores which on culturing were found to be viable.

When the isolates of *C. ulmi* obtained from each of the four sample logs were compared with twelve British aggressive isolates (four from each of the three main outbreak areas¹) and four non-aggressive isolates from Moreton in Oxfordshire, they fell into the fluffy group on the basis of growth rate (Table 1) and culture morphology (Fig. 3). It was evident that *C. ulmi* from the Canadian elm logs closely resembled the aggressive strain of the fungus present in this country.

Rock elm is a North American species found chiefly in the Great Lakes region², its distribution in Canada being confined to south-east Ontario (Fig. 4). As its name implies, it produces an extremely hard timber, which is much favoured on account of its straight grain, great density and strength. It seems that for about one hundred years Rock elm logs with bark attached have been imported into Britain from Canada, principally for boat building. Dutch elm disease entered the Toronto region from the United States by way of Windsor and the Niagara peninsula in 1950 (ref. 3). By the early 1960s the disease was present throughout south-east Ontario, and the importation into Britain of the disease on logs of Rock elm from any point in the range of this species in Canada was a possibility. *Hylurgopinus rufipes* is present throughout southern Ontario (Fig. 4) and the small European bark beetle *Scolytus multistriatus* is also present, with a range similar to that of Rock elm⁴.

Investigation of the trade has shown that the chief ports of entry into England during the 1960s were Avonmouth, Liverpool, London and Southampton, and among the chief centres of usage were Chatham, Plymouth and Portsmouth. At that time the logs were not in containers, but were carried as deck or loose cargo. From May to September bark beetles, whether *H. rufipes* or *S. multistriatus* carrying spores of *C. ulmi*, could be expected to emerge from infested logs similar to those we examined, either at the docks or in transit to boat and timber yards. They would then tend to migrate to adjacent healthy elms for maturation feeding, thus initiating infection. It is further possible that logs arriving in this country could have afforded breeding material for native *Scolytus* species which could then have picked up the Canadian strains of the fungus. Once introduced into our native elms, the aggressive strain

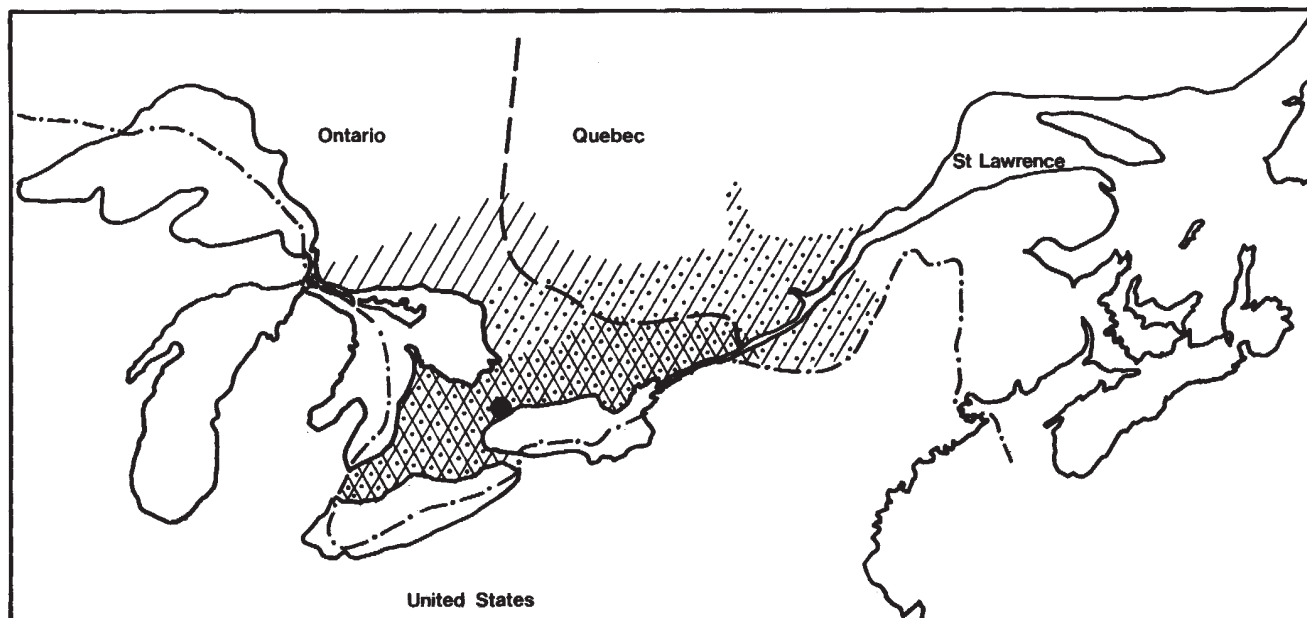


Fig. 4 Distribution of Rock elm (*Ulmus thomasii*, ▨) in Canada, together with the distribution of *Hylurgopinus rufipes* (▩) and Dutch elm disease (▤) in the Toronto (●) area in 1963. (Adapted from refs. 2, 3 and 4.)

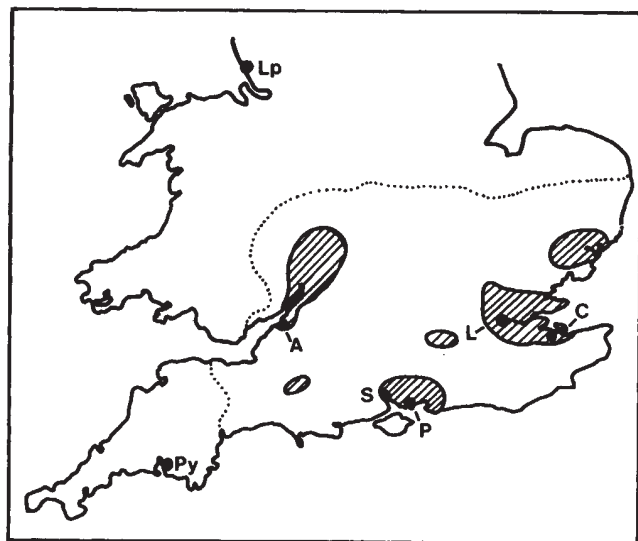


Fig. 5 Map of southern Britain showing the relationship between the chief areas of Dutch elm disease identified in the 1971 survey, and ports of entry and centres of usage of Rock elm. . . . Principal area covered by survey; ■, areas worst affected by disease; A, Avonmouth; C, Chatham; L, London; Lp, Liverpool; P, Portsmouth; Py, Plymouth; S, Southampton.

would, by its lethal nature, increase the locally available beetle breeding material. This in turn would result in rapid development of the disease outbreaks to epidemic proportions.

The ports and centres of usage mentioned above and the principal disease areas identified in the 1971 Forestry Commission Survey of southern England⁵ are shown in Fig. 5. It seems that disease in two of these areas, namely that on the south coast and that around the Thames Estuary, could well be associated directly with sites of importation and usage. Other disease centres such as that in the Severn Vale could have originated during the transport of imported logs by road or rail to sites where they were to be cut. In the past few years new disease centres have almost certainly arisen as a result of the transport of infected native elm material into regions previously free from the aggressive strain.

We conclude that the current epidemic of Dutch elm disease has most probably resulted from the importation of diseased elm logs carrying aggressive North American strains of *C. ulmi*. It therefore seems that two distinct groups of *C. ulmi* identified by sampling in 1972 (ref. 1) represented elements of two populations hitherto geographically isolated: one aggressive, originating from within the North American population, and probably responsible for the continued activity of the disease on that continent; the other non-aggressive, representing the residual population from the previous British epidemic, following its decline during the 1930s (ref. 6).

As perithecia of *C. ulmi* were present on the logs we examined it is apparent that this process of introduction could have resulted in the entry of a range of genetic material, including the two compatibility types. Our preliminary data (unpublished) indicate that both compatibility types are also present among British non-aggressive isolates. There is thus the possibility of outcrossing between the two groups, aggressive and non-aggressive, in nature and hence of the appearance of intermediate phenotypes. We are considering the implications of these findings for our present work on the population dynamics of *C. ulmi* and on the genetic control of pathogenicity.

Movement of elm logs has played an important part in the history of Dutch elm disease, as *C. ulmi* and *Scolytus multistriatus* reached the United States from Europe on elm burl logs imported for the manufacture of veneer⁷. It appears that the same means has now caused a resurgence of the disease in Britain. This event not only emphasizes the continuing danger, stressed by Peace⁸, of the introduction into Britain of

serious tree diseases not now present such as oak wilt (*Ceratomyces fagacearum*), chestnut blight (*Endothia parasitica*) and elm phloem necrosis, but also demonstrates the serious danger of introducing new and more aggressive strains of tree pathogens already present.

We thank Chris Walker of our Entomology Section for identifying and assisting with collection of material of *Hylurgopinus rufipes*. We also thank Miss Jenny Cross and Miss Polly Smith for technical assistance, and Mrs Hazel Geoffrey and John Williams for the preparation of photographs and diagrams.

C. M. BRASIER
J. N. GIBBS

Forestry Commission Research Station,
Alice Holt Lodge, Farnham, Surrey

Received March 13, 1973.

- ¹ Gibbs, J. N., and Brasier, C. M., *Nature*, **241**, 381 (1973).
- ² Little, E. L., *Misc. Pubs. US Dep. Agric.*, **1146** (1971).
- ³ Pomerleau, R., *Bi-mon. Prog. Rep. Forest Ent. Path. Branch*, **20**, 1 (1964).
- ⁴ Finnegan, R. J., and Sippell, W. L., *Bi-mon. Prog. Rep. Forest Ent. Path. Branch*, **20**, 3 (1964).
- ⁵ Gibbs, J. N., and Howell, R. S., *Forest Rec., Lond.*, **82** (1972).
- ⁶ Peace, T. R., *Bull. For. Comm., Lond.*, **33** (1960).
- ⁷ May, C., *Circ. US Dep. Agric.*, **322** (1934).
- ⁸ Peace, T. R., *Pathology of Trees and Shrubs*, chap. 38 (Oxford University Press, 1962).

Evidence for a Tumour-associated Antigen in Human Malignant Melanoma

TUMOUR-directed antibodies are found in the serum of patients suffering from malignant melanoma¹⁻⁵, and so we have attempted to detect tumour-associated antigens in extracts from melanoma tumours and urine of patients.

Preliminary experiments showed that after immunization of rabbits with concentrated dialysed urine of melanoma patients, antisera were obtained which, after absorption, reacted in the double diffusion⁶ or single radial diffusion⁷ test which concentrated urine samples from other melanoma patients, but failed to react with urine samples of normal individuals or patients without neuroectodermal neoplasia. Animals immunized with melanoma extracts produced antisera which after absorption showed identical antibody specificity.

These antisera were prepared by immunizing rabbits at 8 days intervals with two injections of 2 mg of crude antigen in Freund's complete adjuvant in the two hind-foot pads. Each injection consisted of 1.2 ml. of material prepared by mixing 0.6 ml. adjuvant with an equal volume of concentrated dialysed urine or with an equal volume of a Tris-saline extract of a melanoma tumour. Blood was withdrawn 28 days from the start of immunization. The resulting antisera were absorbed with lyophilized normal human plasma, normal human urine, Tris-saline extracts of normal skin and ABO red cell stromata.

The specific antigenic activities of the melanoma urine and of the tumour extracts were then isolated by several chromatographic procedures and by different preparative electrophoreses as follows. The presence of antigenic activity at each stage of the following purification was determined by double diffusion against several specific antisera, prepared as described above.

Tumour tissue was homogenized at 30,000 r.p.m. and 50,000 Hz. Extraction was performed with an equal volume of 0.1 M Tris NaCl buffer, pH 7.5, and the supernatant was lyophilized after centrifugation of the homogenate at 1,500g for 30 min and 20,000g for 60 min. Double diffusion analysis revealed the presence of melanoma activity in this material. An aliquot of 2 g of the lyophilized Tris-saline extract was dissolved in 3 ml. of 0.2 M ammonia and applied to a 'Sephadex G-10' column equilibrated with the same solvent. The first peak was

collected and lyophilized as it contained the whole melanoma activity. A further fractionation of this material on 'Sephadex G-100' eliminated, in the exclusion peak, all the contaminating immunoglobulins and other macromolecules. The melanoma activity was located in the second peak which was collected and lyophilized. This material was then separated into 4 peaks on a 'Sephadex G-75' column, and the melanoma activity located in peaks 1 and 2, which were collected. Immunoelectrophoretic analyses revealed that the main contaminant remaining in this material was albumin, eliminated by Pevikon electrophoresis⁹ under standard conditions (0.01 M veronal buffer, pH 8.6) on a block 4.5 × 20 × 1 cm. After electrophoresis at 115 V and 7 mA for 15 h, the block was cut into fractions of 1 cm, and the melanoma activity located between 9 and 11 cm from the origin, while most of the albumin was at 15 cm from the origin. Finally, Pevikon fractions containing melanoma activity were applied on a 110 ml. electrofocusing column in a sucrose density gradient using a pH range from 3 to 6. Focusing was achieved by electrophoresis at 300 V and 72 h and the fractions containing melanoma activity were located between pH 4.5 and 5.5.

Lyophilized urine samples from melanoma patients were fractionated according to the same purification scheme as described above for tumour extracts, with the only exception that the 'G-100' purification step was omitted. Melanoma activity was also recovered in the fractions between pH 4.5 and 5.5 in the electrofocusing purification step.

Antisera were prepared against these active materials isolated by electrofocusing, from the tumour and the urine of melanoma patients. The antisera did not have to be absorbed as described above. A line of identity was demonstrated, using these antisera in the double diffusion test against different melanoma urine samples. This indicates that all materials used for immunization, even with different isoelectric points, contain the same antigenic determinants. Moreover, they must be of similar sizes because they were not separated by gel filtration and block electrophoresis. Preliminary molecular weight determinations by polyacrylamide gel electrophoresis⁹ showed that these antigen(s) have molecular weights between 40,000 and 60,000 and would therefore display beta mobility in Pevikon electrophoresis. It is not clear what the differences are between these similar or identical antigens. Gel electrofocusing¹⁰⁻¹² experiments on antigens purified by preparative electrofocusing showed that these antigens are very rapidly degraded.

All antisera produced were screened against a large panel of different concentrated urine samples. Twenty-nine out of thirty-two samples from melanoma patients reacted positively (Table 1), while sixty-nine out of seventy-two control urine samples gave negative reactions. The three "non-melanoma" urine samples with positive reactions were collected from two patients with neuroblastoma and from a patient with ganglioneuroma. Positive melanoma urine samples were collected

Table 1 Results of Screening Urine Samples with Rabbit Anti-Melanoma Antiserum

Source of urine samples	Total No. of patients	Reaction in double diffusion against anti-melanoma antiserum	
		Positive (+)	Negative (-)
Melanoma	32	29	3
Carcinoma	20	0	20
Myeloma	7	0	7
Other tumours*	8	3†	5
Various non-malignant diseases	24	0	24
Nephropathy	4	0	4
Healthy control	9	0	9
Total "non-melanoma"	72	3	69

* 2 Sarcomas, 1 Wilms's tumour, 2 neuroblastomas, 1 ganglioneuroma, 2 pheochromocytomas.

† 2 Neuroblastomas, 1 ganglioneuroma.

Table 2 Number of Positive Melanoma Urine Samples of Different Stages of the Disease

Type of tumour	Total No. of patients	Positive reaction against anti-melanoma antiserum (+)
Primary melanotic*	13	12
Primary amelanotic*	1	1
General melanotic†	17	15
General amelanotic†	1	1

* Malignant melanomas confined to primary anatomical site.

† Malignant melanomas spread beyond the regional lymph nodes.

from individuals with melanotic or amelanotic tumours (Table 2) in all three stages of the disease described by Lewis *et al.*².

Our experiments, so far, indicate the presence of a common antigen in the tumour which is excreted in the patients' urine. However, the question remains as to whether these tumour-associated antigen(s) are tissue-specific or tumour-specific. It is also unknown whether the common tumour associated antigen(s) of melanomas are of embryonic or viral type. A possible embryonic origin, however, is suggested by the fact that the three positive "non-melanoma" urine samples were from two neuroblastomas and one ganglioneuroma. The precursor cells of all three of these tumours are thought to originate in the neural tube.

This investigation was supported by grants from the Swiss National Foundation for Scientific Research.

S. CARREL*

L. THEILKAES

*Institute for Clinical and Experimental Cancer Research,
University of Berne, 3004 Berne, Switzerland*

Received November 16, 1972; revised January 15, 1973.

* Present address: Institut de Biochimie, Université de Lausanne, 1011 Lausanne.

- Morton, D. L., Malmgren, R. A., and Holmes, E. C., *Surgery*, **64**, 233 (1968).
- Lewis, M. G., Ikonopisov, R. L., Nairn, R. C., Phillips, T. M., Fairley, G. H., Bodenham, D. C., and Alexander, P., *Brit. Med. J.*, **3**, 547 (1969).
- Lewis, M. G., *Rev. Inst. Pasteur (Lyon)*, **3**, 177 (1970).
- Fossati, G., Colnaghi, M. I., Della Porta, G., Cascinelli, N., and Veronesi, U., *Int. J. Cancer*, **8**, 344 (1971).
- Hellström, K. E., and Hellström, I., *Ann. Rev. Med.*, **23**, 19 (1972).
- Ouchterlony, O., *Acta Path. Microbiol. Scand.*, **26**, 507 (1949).
- Manani, G., Carbonara, A. O., and Heremans, J. F., *Immunochimistry*, **2**, 235 (1965).
- Kunkel, H. G., and Slater, R. J., *Proc. Soc. Exp. Biol. Med.*, **80**, 42 (1952).
- Weber, K., and Osborn, M., *J. Biol. Chem.*, **244**, 4406 (1969).
- Fawcett, J. S., *FEBS Lett.*, **50**, 81 (1968).
- Leaback, D. H., and Rutter, A. C., *Biochem. J.*, **108**, 19 P (1968).
- Awdeh, Z. L., Williamson, A. R., and Askonas, B. A., *Nature*, **219**, 66 (1968).

6-Azauridine-5'-phosphoric Acid: Unusual Molecular Structure and Functional Mechanism

6-AZAURIDINE has been used successfully in the treatment of leukaemia¹. The nucleoside is not effective as such but is converted *in vivo* by the enzyme uridine kinase to 6-azauridine-5'-phosphate, which inhibits the enzyme orotidylic acid decarboxylase²⁻⁴. This inhibition stops the metabolism of orotidine-5'-phosphate to uridine-5'-phosphate, that is the *de novo* synthesis of uridylic acid (Fig. 1).

In crystalline state and in solution, 6-azauridine occurs in the *anti* conformation with the ribose moiety in the C(3')-endo envelope form and the conformation about the C(4')-C(5') bond is not the usual *gauche*, *gauche* with atom O(5') positioned above the ribose ring but rather the *gauche*, *trans* form^{5,6}.

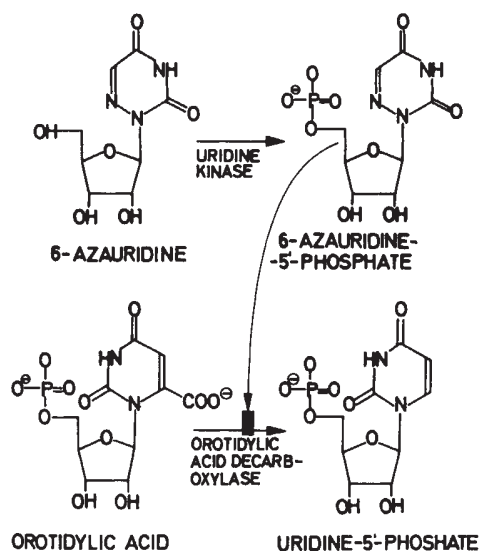


Fig. 1 Mechanism of inhibition of orotidylic acid decarboxylase by 6-azauridine.

(Fig. 2). Our X-ray study on 6-azauridine-5'-phosphate was undertaken in order to establish whether it assumes a similar conformation as 6-azauridine or whether it occurs in the normal *gauche, gauche* form found for all the nucleotides investigated so far^{7,8}.

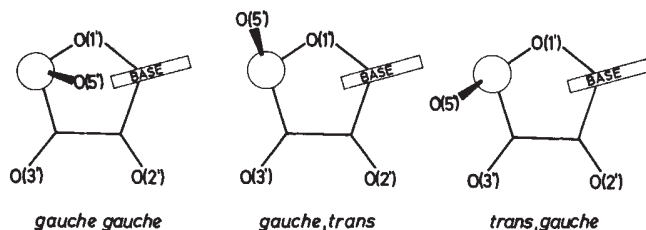


Fig. 2 Conformation about the nucleoside C(4')-C(5') bond.

6-Azauridine-5'-phosphoric acid was crystallized from isopropanol/water as thin needles with orthorhombic lattice symmetry, space group $P2_12_12_1$, and cell constants $a=20.615$ Å, $b=6.265$ Å, $c=11.881$ Å. We concluded from a measurement of the crystal density that the asymmetric unit contained the trihydrate of 6-azauridine-5'-phosphoric acid. The structure was solved from diffractometer data by application of Patterson methods and refined by full matrix least squares methods to a crystallographic discrepancy index $R=7\%$. A difference Fourier synthesis computed at this stage revealed no serious discrepancies with the derived model.

Bond angles and distances within the 6-azauridine-5'-phosphoric acid molecules (Fig. 3) are comparable to those observed for 6-azauridine and for other nucleoside-5'-phosphates and will be discussed elsewhere, together with a full account of this structure. The location of the acid hydrogen ion is not obvious from the latest difference Fourier synthesis. Yet it is certain that the 6-azauracil moiety in 6-azauridine-5'-phosphoric acid is uncharged as its bond distances and angles are identical within experimental errors with the values found in 6-azauridine.

The conformation of 6-azauridine-5'-phosphoric acid about the glycosidic C(1')-N(1) linkage is *anti* with the torsional angle C(2')-C(1')-N(1)-N(6) = -33° (that is, the C(2)=O(2) keto group is pointing away from the ribose ring). The ribose moiety is in a rather symmetrical C(2')-*exo*, C(3')-*endo* half chair form. The conformation about the C(4')-C(5') bond is

not *gauche, gauche* as in all the nucleoside-5'-phosphates so far studied but rather *gauche, trans* as in 6-azauridine, with torsional angles O(1')-C(4')-C(5')-O(5'), 71° ; and C(3')-C(4')-C(5')-O(5'), -172° .

To illustrate the change in molecular geometry caused by the change in conformation from *gauche, gauche* to *gauche, trans* we have depicted in Fig. 4 the X-ray results obtained for this structure and for uridine-5'-phosphate⁹, which was found to occur in the *gauche, gauche* conformation. When the ribose atoms C(1'), O(1') and C(4') are placed in coincidence, the phosphorus atoms are separated by 4.25 Å.

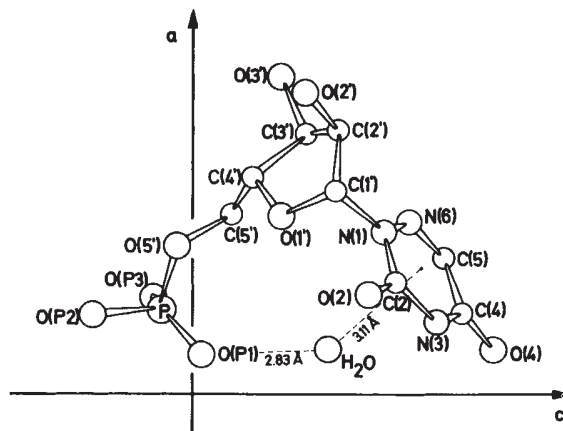


Fig. 3 The molecular structure of 6-azauridine-5'-phosphoric acid viewed down the b -axis. Two of the three water molecules within the asymmetric unit are omitted.

The two molecules shown in Fig. 4 are chemically different only in position 6 of the heterocyclic base. Extended Hückel calculations revealed that in 6-azauridine the N(6) atom accumulates a charge of $-0.47 e$, compared with a charge of $+0.28 e$ at atom C(6) of uridine¹⁰. Thus, it appears that the *gauche, trans* conformation of 6-azauridine as well as of 6-azauridine-5'-phosphoric acid is due to an electrostatic repulsion between N(6) and O(5'). Furthermore, in 6-azauridine-5'-phosphoric acid the hydrogen bond interaction C(6)-H...O(5') discussed for uridine-5'-phosphate derivatives¹¹ cannot stabilize the *gauche, gauche* conformation.

As the structure of orotidylic acid has not yet been determined, the reason why 6-azauridine-5'-phosphate is an inhibitor for orotidylic acid decarboxylase must be derived by analogy. It is known that orotidine occurs in the *syn*-conformation due to the bulky carboxyl group at the base C(6) position^{12,13} and exhibits, in solution, a *gauche, trans* or *trans, gauche* conforma-

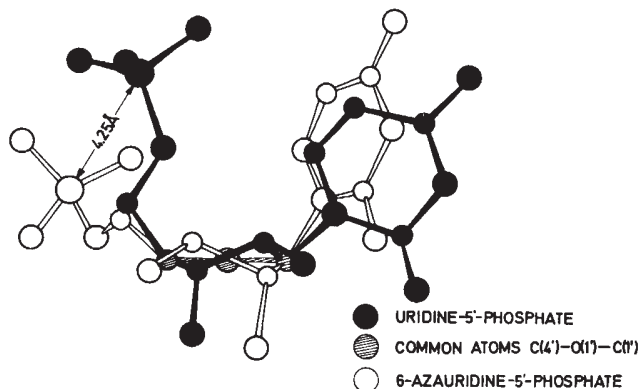


Fig. 4 Comparison of the *gauche, gauche* and *gauche, trans* structure of uridine-5'-phosphate and 6-azauridine-5'-phosphoric acid respectively.

tion about the C(4')-C(5') bond¹⁴. Furthermore, a *gauche*, *trans* conformation has been observed for a crystalline O(5') ester analogue in the *syn* conformation, 2'-deoxy-2'-fluorouridine-3',5'-diacetate (Fig. 5) (D. S., W. S., and P. Main, in preparation).

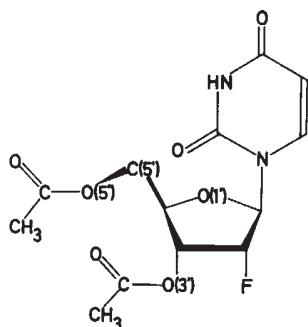


Fig. 5 Chemical structure of 2'-deoxy-2'-fluorouridine-3',5'-diacetate.

Thus, one would expect orotidylic acid to occur in *syn* and *gauche*, *trans* conformations as well. This peculiar molecular structure would explain why 6-azauridine-5'-phosphate with similar *gauche*, *trans* orientation of the phosphate group acts as an inhibitor for orotidylic acid decarboxylase: the anti-metabolite mimics the metabolite and is bonded to the active site of orotidylic acid decarboxylase. This also suggests why other pyrimidine nucleosides which are not substituted in the base 6-position do not act as inhibitors: they occur in normal *anti* and *gauche*, *gauche* conformations.

Another conclusion derived from this study concerns the enzyme uridine kinase. As both substrate and reaction product exhibit an unusual *gauche*, *trans* conformation, the enzyme is either not very specific, or it forces the substrate 6-azauridine during bonding at the surface of the active site to assume a "normal" *gauche*, *gauche* conformation like uridine.

We thank F. Cramer for his interest; W. S. acknowledges discussions on the 6-azauridine structure with Dr F. E. Hruska, B. Pullman, A. Saran, and M. Sundaralingam. This work was supported in part by the Deutsche Forschungsgemeinschaft.

W. SAENGER
D. SUCK

Max-Planck-Institut für Experimentelle Medizin,
Abteilung Chemie,
Göttingen

Received November 10, 1972; revised January 25, 1973.

¹ Handschumacher, R. E., Calabresi, P., Welch, A. D., Bono, V., Fallou, H., and Frei, E., *Cancer Chemotherapy Rep.*, **21**, 1 (1962).

² Skoda, J., *Progress in Nucleic Acid Research*, **2**, 197 (1963).

³ Hernandez, K., Pinkel, D., Lee, S., and Leone, L., *Cancer Chemotherapy Rep. Part I*, **53**, 203 (1969).

⁴ Suhadolnik, R. S., *Nucleoside Antibiotics* (Wiley Interscience, New York, 1970).

⁵ Schwalbe, C. H., Saenger, W., and Gassmann, J., *Biochem. Biophys. Res. Commun.*, **44**, 57 (1971).

⁶ Hruska, F. E., *The Jerusalem Symposium on Quantum Chemistry and Biochemistry*, **5** (edit. by Bergmann, E. D., and Pullman, B.) (The Israel Academy of Sciences and Humanities, Jerusalem, 1972).

⁷ Sundaralingam, M., *The Jerusalem Symposium on Quantum Chemistry and Biochemistry*, **5** (edit. by Bergmann, E. D., and Pullman, B.) (The Israel Academy of Sciences and Humanities, Jerusalem, 1972).

⁸ Arnott, S., *The Jerusalem Symposium on Quantum Chemistry and Biochemistry*, **4** (edit. by Bergmann, E. D., and Pullman, B.) (The Israel Academy of Sciences and Humanities, Jerusalem, 1972).

⁹ Shefter, E., and Trueblood, K. N., *Acta Cryst.*, **18**, 1067 (1965).

¹⁰ Schwalbe, C. H., and Saenger, W., *J. Mol. Biol.* (in the press).

¹¹ Seeman, N. C., Sussman, J. L., Berman, H. M., and Kim, S. H., *Nature New Biology*, **223**, 90 (1971).

¹² Hruska, F. E., *J. Amer. Chem. Soc.*, **93**, 1759 (1971).

¹³ Suck, D., and Saenger, W., *J. Amer. Chem. Soc.*, **94**, 6520 (1972).

¹⁴ Hruska, F. E., *J. Amer. Chem. Soc.*, **93**, 4334 (1971).

The Identification of Labelled Structures on Autoradiographs

THERE is no agreed method of deciding whether or not a given structure is radioactive from its autoradiograph. Some authors require that a cell, for instance, should have five or more overlying grains to be considered labelled, whereas others are satisfied with three or four grains. By setting such an arbitrary limit, the experimenter risks accepting as labelled some non-radioactive cells which, by chance, have a higher number of background grains over them, and risks rejecting as unlabelled those radioactive cells which have failed to give rise to the requisite number of grains over them. In an ideal preparation with very low background and high grain densities over radioactive structures, the error introduced by this arbitrary definition of labelling is likely to be small. The smaller the differences between the mean grain densities over background and over the radioactive sources, however, the greater this error becomes.

This problem has been recognized and solutions have been proposed in several previous publications¹⁻⁵. These all tend to give similar values for the percentage of a given population of structures that is radioactive, when the grain densities over labelled sources are high, but at mean grain densities approaching background levels the values derived by these methods diverge considerably.

Here we present an analytical method which is simple and direct to use, and accurate even when some of the radioactive cells have no grains over them at all. We start from the assumption that grain densities over background areas and over radioactive sources both have a Poisson distribution. This assumption is generally valid⁶, though exceptions do occur.

In order to make allowance for cells which produce no grains it is necessary to make more than one measurement of the radioactive cells, and to compare these with measurements made from similar areas of emulsion giving values for background: these may be from areas on the same autoradiographs away from the radioactive cells or non-radioactive cells autoradiographed under identical conditions, as appropriate. The two measurements we have chosen are the percentage of cells with one or more grains over them, and the mean number of grains per cell.

Having obtained the measurements, one then calculates *A*, *B*, and *C* as follows.

$$A = 100 - (\% \text{ non-radioactive cells with one or more grains})$$

$$B = (\% \text{ radioactive cells with one or more grains}) - (\% \text{ non-radioactive cells with one or more grains})$$

$$C = (\text{mean number of grains per radioactive cell}) - (\text{mean number of grains per non-radioactive cell})$$

The true percentage of labelled cells, *D*, is then given by

$$B/AC = D/100 C [1 - \exp(-100 C/D)]$$

Rather than solve this equation for each experimental result, Fig. 1 can be used. We first calculate *B/AC* and locate this value on the vertical scale, then read across to intersect with the curve, and finally down to the corresponding value on the horizontal scale, which gives *100 C/D*. We have already calculated *C*, so we can now evaluate *D*.

We have chosen an experiment to illustrate our method in which the random or background contribution to the observed grain densities was high, and the level of labelling over radioactive sources likely to be low. Rat thoracic duct lymphocytes were incubated with ¹²⁵I-labelled normal rabbit globulin,

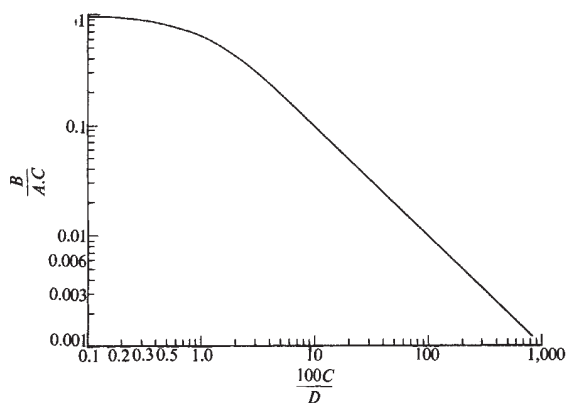


Fig. 1 Scale to determine the true percentage of labelled cells.

washed, centrifuged and resuspended three times in non-radioactive buffer, then smeared on slides and autoradiographed by dipping in Ilford G5 emulsion⁷. Background measured on areas of emulsion distant from cells on these slides was higher than on autoradiographs of non-radioactive suspensions, partly due to the presence of traces of ^{125}I in the suspending medium and partly to the gamma and X-ray components of the emission of ^{125}I . Our background areas were therefore taken from the experimental slides, away from underlying cells. Scintillation counting experiments revealed that on average each lymphocyte was producing one disintegration every 4 days. The experiment was designed to show if all lymphocytes in this sample were equally radioactive, or if a sub-group possessed the ability to bind immunologically non-specific globulins.

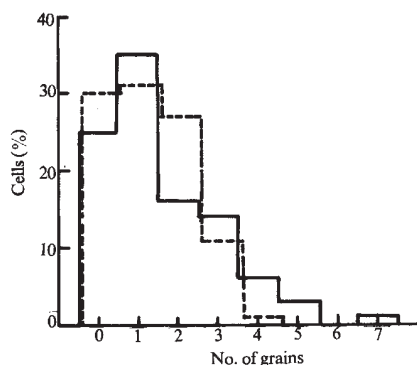


Fig. 2 Rat thoracic duct lymphocytes incubated with ^{125}I -labelled rabbit globulin. ---, Emulsion remote from labelled cells; —, labelled cells.

The results are shown in Fig. 2: the grain counts over cells are low, mostly overlapping those from background areas. From our results $A=100-70=30$, $B=75-70=5$, $C=1.55-1.22=0.33$ and $B/A/C=0.505$; from Fig. 1 this value of $B/A/C$ gives $100\ C/D=1.57$, so that knowing $C=0.33$, $D=21\%$.

This example shows that large numbers of cells may be labelled even though most of the cells have no grains or only one or two grains overlying them. It is important to emphasize that extensive counting must be performed. Calculating A , B and C involves calculating differences between observations, so that small errors in the observations can mean large errors in the differences. These errors can be minimized by keeping A , B and C as large as possible by using the most sensitive methods to detect the radioactivity in the labelled cells, and by ensuring that the background is low in the unlabelled areas. It is difficult to suggest a simple method of determining how many areas to count in any given experiment. If the experi-

mental and background distributions overlap, and a highly accurate final answer is needed, repeated sampling with 200 cells per experiment will generate data the reproducibility of which will indicate the total number of counts required.

This approach should avoid the necessity of setting up arbitrary definitions of a labelled cell. All such definitions introduce considerable errors by excluding labelled cells which either produce no grains or do not produce the requisite number of grains. Using our analytical procedure these problems are avoided, and a correction can be made for radioactive background without recourse to elaborate computer programs. The present correction approximates to Stillström's⁴ when the cells are very radioactive.

J. M. E. is supported by a Medical Research Council fellowship. The labelled cells in the example quoted were kindly provided by Dr A. F. Williams, MRC Immunochemistry Unit, Oxford.

J. M. ENGLAND*

Department of Haematology,
Northwick Park Hospital and Clinical Research Centre,
Watford Road,
Harrow, Middlesex

A. W. ROGERS

MRC Neuroendocrinology Unit,
Department of Human Anatomy,
University of Oxford,
South Parks Road,
Oxford

R. G. MILLER, JUN

Department of Community and Preventive Medicine,
Stanford University School of Medicine, Stanford,
California 94305

Received November 24, 1971; revised November 30, 1972.

* Present address: Experimental Haematology Research Unit, St Mary's Hospital Medical School, London W2.

- ¹ Sawicki, W., Blaton, O., and Rowinski, J., *Histochemie*, **26**, 67 (1971).
- ² Moffatt, D. J., Youngberg, S. P., and Metcalf, W. K., *Cell Tissue Kinet.*, **4**, 293 (1971).
- ³ England, J. M., and Miller, R. G., *J. Microsc.*, **92**, 167 (1970).
- ⁴ Stillström, J., *Inter. J. Appl. Radiat. Isotopes*, **14**, 113 (1963).
- ⁵ Bresciani, F., and Thompson, K., *Rep. Brookhaven, Nat. Lab.*, BNL-8360 (1963).
- ⁶ England, J. M., and Rogers, A. W., *J. Microsc.*, **92**, 159 (1970).
- ⁷ Rogers, A. W., *Techniques of Autoradiography* (Elsevier, Amsterdam, 1967).

Microspectrophotometry of Trypsin-Leishman-stained Human Chromosomes

RECENTLY new chromosome banding techniques have become available, by means of which each chromosome in the human karyotype can be identified¹⁻³. To illustrate graphically quinacrine (Q)-stained metaphase chromosomes, Caspersson *et al.* developed a technique using densitometric measurements from photographs³. Diagrams of differential fluorescence have been computerized, and a method for automatic karyotyping is being developed⁴⁻⁶.

To measure the number, position, relative size and intensity of bands produced by stains other than Q, and to avoid photographs of the banding patterns, we have produced diagrams from trypsin-treated chromosomes stained with Leishman (TL) using microspectrophotometry (MSP) directly on the specimen. Human metaphase chromosomes from lymphocytes⁷ were stained with a modified trypsin-Leishman method¹.

Six suitable metaphase figures with chromosomes of different degree of contraction from four individuals were selected. The absorption pattern of each chromosome was registered by means of the microspectrophotometer UMSP-I (Zeiss) at a wavelength of 545 nm (absorption maximum, Leishman-stained regions). A measuring area with a diameter of 0.3 μ m was selected, and light absorbance of this area in continuous steps down the midline of the chromosome was graphically or digitally recorded⁸.

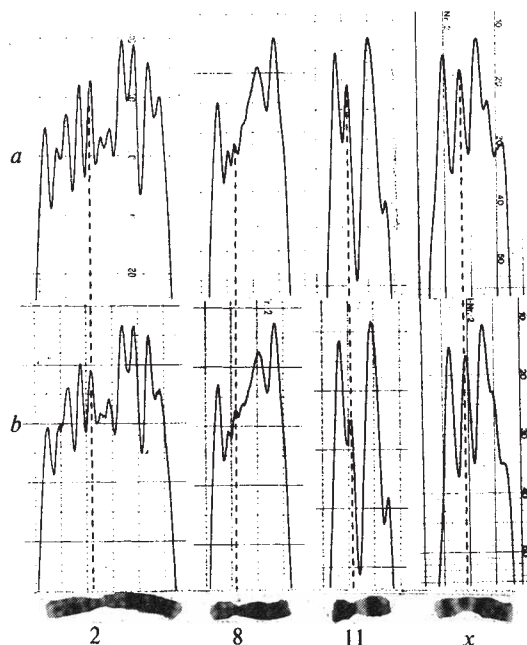


Fig. 1 Chromosomes 2, 8, 11 and X; corresponding diagrams were measured twice on different occasions to illustrate the reproducibility of the diagrams. All chromosomes are placed with the short arm to the left. ---, Position of the centromere.

Each chromosome was measured four times, twice from one end and twice from the other end of the chromosome. Repeated measurements were performed at different times. The centromere position and the chromosome lengths were measured with a micrometer.

Fig. 1 shows the reproducibility of the banding patterns from four different chromosomes recorded directly from the slides. All information on the slide, here illustrated on photomicrographs, is found on the diagrams.

Both diagrams from, for example, chromosome No. 8 have three rather large spikes, all similar in size, position and shape and corresponding to the three dark bands on the photomicrograph. Three smaller but somewhat variable spikes around the centromere correspond to indistinct bands on the photomicrograph.

Chromosome 11 has a broad light band followed by a broad dark band on the long arm. These two bands correspond in both diagrams to a deep wave followed by a large spike.

On the X chromosome are two dark bands, one on each side of the centromere (here heavily stained), and further distally on the long arm are more indistinct bands. A large spike corresponding to each of the two dark bands, a spike corresponding to the dark centromere region and two small spikes which are more pronounced on (a) than on (b) are evident on the diagrams.

Fig. 2 shows diagrams of the same chromosome from four different metaphases. The photomicrographs show some variations between different chromosomes 1 (top) concerning contraction, number of bands and staining intensity. On 1 (a) and (c) two proximal dark bands on the short arm are

clearly illustrated on the corresponding diagrams. On 1 (b) the most proximal of the two dark bands is fairly indistinct, corresponding to a weakly visualized spike. On 1 (d) there is only one broad band in the proximal part of the short arm. This corresponds to a broad spike. The dark band on the proximal part of the long arm is on all diagrams. On the distal part of the long arm, depending on the degree of contraction, three or four dark bands are present, corresponding to the number of spikes on the diagrams.

Chromosome 7 (bottom) has three dark bands: one in the distal part of the short arm and two in the middle of the long arm. These bands correspond to large spikes on the diagrams. The proximal part of the short arm and the centromere region are variably stained; these features are illustrated as variable spikes.

Spikes and bands corresponded to each other with regard to position, size and shape; thus each chromosome had a characteristic diagram.

The difficulties in sweeping the measuring area exactly down the midline of the chromosome were responsible for the slight deficiencies in the reproducibility of the diagrams and these, together with differences in staining intensity and degree of contraction of the chromosomes, contributed to the variations of diagrams from homologous chromosomes and identical chromosomes from different metaphases. Further standardization of preparation and staining will probably improve matters.

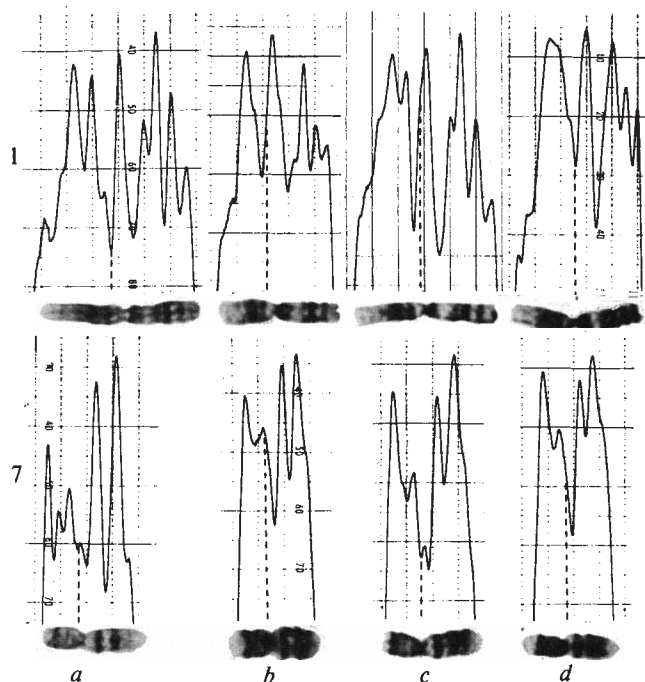


Fig. 2 Chromosomes 1 (top) and 7 (bottom) from four different metaphases with corresponding diagrams developed directly from the slide. Small variations in the diagrams are caused by corresponding variations in the banding patterns illustrated on the photomicrographs. Note the polymorphism of the darkly stained area on the long arm near the centromere on chromosome 1.

The UMSP used has a circular measuring area. Varying diameters from 0.3 to 1.0 μ m showed that smaller areas give more detailed diagrams. As chromosomes are 1 to 2 μ m wide only a portion of the whole chromosome is measured. TL-stained chromosomes often show banding across the whole chromosome, but variations occur, especially in light bands. By measuring only a fraction of a band, variation across the chromosome may lead to differences in absorption patterns.

A slit aperture $0.3 \times 2.0 \mu\text{m}$ might give more constant diagrams³.

The amount of stain bound varies with circumstances, so TL staining does not meet the requirements of stoichiometry. Further investigations are necessary. For identification, however, this is of secondary importance. The aim is to determine the location of stain maxima and minima. The position of the centromere on diagrams can be determined either directly in the microscope or on the photomicrographs. Methods for detecting the centromere by means of the instrument are being investigated. Curved chromosomes are divided into several parts with different axes. Microspectrophotometry has been used to establish a technique for accurate, objective, automatic recording of banding patterns of TL-stained metaphase chromosomes. Diagrams are constant and characteristic. Identification of chromosomes and diagnosis of the exact nature of abnormalities seem possible (ref. 9 and C. L., J. P. and S. Vestermark, unpublished).

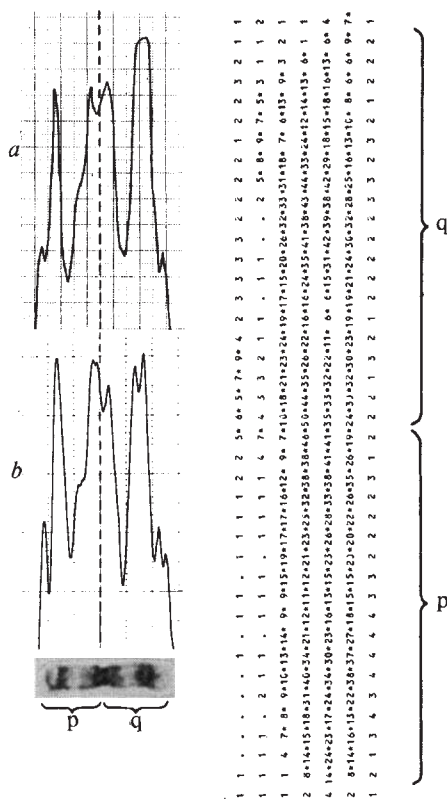


Fig. 3 Right: part of a digitally recorded metaphase (chromosome 3, p: short arm, and q: long arm); this chromosome has been converted manually to a diagram (a) by combining the figures along the longitudinal direction of the chromosome. b, The same chromosome illustrated graphically by the technique described in the text.

The technique could be very useful for performing comparative studies with other banding techniques. Diagrams obtained after Q staining¹⁰ and the patterns described are quite similar, but the diagrams shown here seem more distinct and detailed, implying that with the TL technique bands are better defined. Another advantage is that measurements can be made directly from the slide.

A possible future application of the technique will be automatic identification of the karyotype. A modified UMSP can be used for digital recording and thus a chromosome or a whole metaphase can be recorded and karyotyped by computer. Such studies are in progress.

Fig. 3 shows part of a digitally recorded metaphase which

has been converted manually to a diagram similar to that obtained with the technique described.

This work was supported by the Danish Medical Research Council and a Steners grant.

C. LUNDSTEEN
P. ERNST
J. PHILIP

Chromosome Laboratory,
Department of Obstetrics and Gynaecology
and Department of Medicine, A,
Rigshospitalet,
University of Copenhagen

Received November 7; revised November 28, 1972.

- ¹ Seabright, M., *Lancet*, ii, 971 (1971).
- ² Sumner, A. T., Evans, H. J., and Buckland, R. A., *Nature New Biology*, **232**, 31 (1971).
- ³ Caspersson, T., Zech, L., Johansson, C., and Modest, E. J., *Chromosoma*, **30**, 215 (1970).
- ⁴ Caspersson, T., Lomakka, G., and Møller, A., *Hereditas*, **67**, 103 (1971).
- ⁵ Møller, A., Nilsson, H., Caspersson, T., and Lomakka, G., *Exp. Cell Res.*, **70**, 475 (1972).
- ⁶ Caspersson, T., Castleman, K. R., Lomakka, G., Modest, E. J., Møller, A., Nathan, R., Wall, R. J., and Zech, L., *Exp. Cell Res.*, **67**, 233 (1971).
- ⁷ Moorhead, P. S., Nowell, P. C., Mellman, W. J., Battips, D. M., and Hungerford, D. A., *Exp. Cell Res.*, **20**, 613 (1960).
- ⁸ Trapp, L., *Introduction to Quantitative Cytochemistry*, 427 (Academic Press, New York, 1966).
- ⁹ Lundsteen, C., and Philip, J., *Cytogenetics* (in the press).
- ¹⁰ Caspersson, T., Lomakka, G., and Zech, L., *Hereditas*, **67**, 89 (1971).

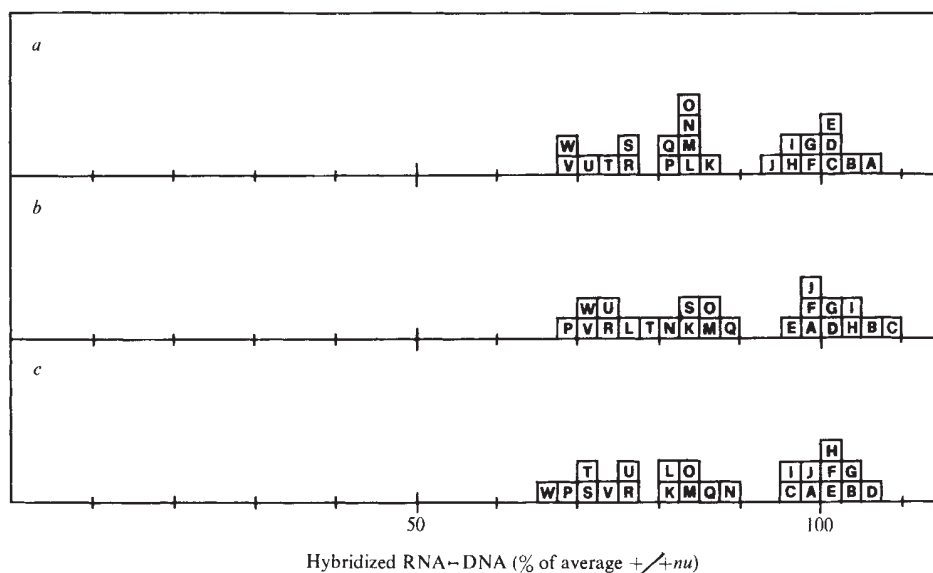
Nuclear DNA Content Variability in *Xenopus laevis*: A Redundancy Regulation common to all Gene Families

IN an earlier communication¹ we presented data about the number (R) of redundant rRNA genes in somatic cells of several species of amphibians. We also determined the number (A) of extrachromosomal nucleoli produced during oogenesis by genetic amplification. Both R and A are constant for each species and vary among different species falling into categories following a geometrical progression (A and $R=2^n$).

To account for these data and other known facts about rRNA genes, we proposed a model in which the behaviour of these genes during amplification is analogous to that of the genome of inducible viruses. During meiosis each rRNA gene of the nucleolar organizer is excised from the chromosome in the form of a circular monogenic DNA molecule. Each circle replicates, during the amplification phase, doubling its length at each replication round. After a number (n) of rounds, one of the amplified circles (containing the 2^n rRNA genes) is re-integrated into the chromosome to restore the lost cluster of redundant genes, while the others give rise to the extrachromosomal nucleoli. This model accounts for genetic rectification of the redundant rRNA genes: their homogeneity will be maintained without excluding the possibility of evolutionary changes in the whole family. One implication of this model is that the number of redundant rRNA genes is established anew at each meiosis by some kind of regulation.

Other genes known to be redundant are the 5S RNA², 4S RNA² and histone³ genes, and it is to be expected that more redundant genes will be demonstrated, for a significant proportion of the eukaryotic genome is made up of repetitious DNA sequences². Many years ago Callan⁴ proposed that most if not all genes are redundant, on the basis of cytological observations. All these redundant genes would require an efficient mechanism of rectification such as the one we proposed for the rRNA gene family¹. In this case, regulation would be necessary to establish the number of redundant genes of each family to be reintegrated into the genome. This regulation

Fig. 1 Hybridization of DNA prepared from $+/+ nu$ and $+p nu$ *Xenopus laevis* with rRNA (a), 5S RNA (b) and 4S RNA (c). DNA was extracted from livers of ten $+/+ nu$ (A-J) and thirteen $+p nu$ (K-W) animals selected as described in Table 1 among the progeny of two normal parents ($+/+ nu$). DNA extracted as previously described¹ was further purified on preparative CsCl density gradients. ³H-labelled rRNA, 5S RNA and 4S RNA were obtained from a *Xenopus* cell line. Method of culture, labelling conditions, purification of rRNA by sucrose gradients and of 5S RNA and 4S RNA by 'Sephadex' columns have been described^{7,8}. Molecular hybridization was carried out in triplicate at saturating conditions in $2 \times SSC$ at $65^\circ C$ for 16 h at a ³H-RNA concentration of $3 \mu g ml^{-1}$ (rRNA), $1 \mu g ml^{-1}$ (5S RNA), $3 \mu g ml^{-1}$ (4S RNA) as described¹. After the filters had been counted, the amount of DNA retained on them was assayed⁹. Saturation levels are given as % of the average value obtained for $+/+ nu$ animals.



can either be independent for each gene family or, more simply, common to all of them. This can easily be tested: animals partially deficient in rRNA genes ($+p nu$) are frequently found in natural populations of *Xenopus laevis*⁵. According to our model this condition does not arise from a classical deletion but rather from an alteration of the regulation mechanism. If the hypothesis of a common regulation of redundant gene families were correct, we should expect a parallel decrease of the redundancy number of all gene families: this could be tested by measuring the DNA content of somatic diploid cells or titrating, by RNA-DNA hybridization, gene families other than rRNA—for example 4S RNA and 5S RNA gene families.

Table 1 summarizes the data on total DNA content measured by two different techniques. Individuals of $+/+ nu$ and $+p nu$ genotype were selected on the basis of different nucleolar sizes by phase contrast observation of somatic cells⁵ (all partially defective individuals were classified as $+p nu$ without attempting to distinguish the $p nu$ from the $p^1 nu$ condition⁵. All the $+p nu$ animals show a decrease in total DNA content per diploid cell (a reduction of rRNA genes only would have affected the total DNA content of less than 0.1%) (Table 1). This decrease is not the same for all the $+p nu$ individuals, which seem to fall into discrete categories. A first group of $+p nu$ has a total DNA content around 5.25 pg/diploid cell. A second category has about 4.95 and a third about 4.75 pg/diploid cell. The difference between the normal value ($+/+ nu$) and the first $+p nu$ group is about double the difference between the first and second $+p nu$ groups and about four times higher than that between the second and the third categories $+p nu$, suggesting that the size of gene families is progressively reduced to one half, one quarter or one eighth of normal. From these observed decreases (about 12%, 17% and 20% for the three categories respectively) of the total DNA content per diploid cell in $+p nu$ animals it can be estimated that about 45–50% of the genome of *Xenopus laevis* is made up of gene families the redundancy of which is under common regulation. This finding is in good agreement with the known fact that about 50% of the genome of *Xenopus laevis* is made up of repetitious DNA sequences⁶.

Alternatively, we used DNA from $+/+ nu$ and $+p nu$ *Xenopus laevis* for molecular hybridization experiments to measure the redundancy of 5S RNA and 4S RNA genes. Fig. 1 summarizes all data obtained hybridizing at saturation DNA separately extracted from 10 $+/+ nu$ and 13 $+p nu$ individuals with ³H-labelled 5S RNA, 4S RNA and, as control,

rRNA (28S+18S). As expected, the saturation level for rRNA (Fig. 1a) is reduced for all $+p nu$ DNA with respect to the normal $+/+ nu$. A parallel reduction was observed for 5S

Table 1 DNA Content (pg/Diploid Cell) in $+/+ nu$ and $+p nu$ *Xenopus laevis*

	Individual No.	Indole DNA pg/diploid cell	Individual No.	Feulgen histophotometry DNA pg/diploid cell	Average	
					DNA pg/diploid cell	DNA %
$+/+ nu$	1	6.02 ± 0.09				
	2	6.00 ± 0.06				
	3	5.96 ± 0.07				
	4	5.95 ± 0.08	*	5.95	5.95	100
	5	5.90 ± 0.10				
	6	5.87 ± 0.07				
$+p nu$	7	5.34 ± 0.08	13	5.28 ± 0.06		
	8	5.29 ± 0.05	8	5.23 ± 0.04	5.24	88
	9	5.25 ± 0.07	14	5.23 ± 0.07		
	10	5.17 ± 0.07	7	5.15 ± 0.11		
	11	4.96 ± 0.07	15	4.99 ± 0.05		
			16	4.95 ± 0.05	4.95	83
			17	4.94 ± 0.07		
			18	4.92 ± 0.06		
	12	4.77 ± 0.08	19	4.75 ± 0.09		
			12	4.71 ± 0.08	4.73	80
			20	4.70 ± 0.05		

$+/+ nu$ and $+p nu$ animals were selected by phase contrast microscope observation of squashes of buccal mucosa of adult animals from a natural population (1–12). Alternatively selection was done among tadpoles obtained crossing two normal ($+/+ nu$) parents, by examination of squash preparations of the tailtips; the tadpoles were then reared up to about 4 months after metamorphosis and checked again before use (13–20). At least ten DNA determinations for each animal were carried out on carefully counted samples of erythrocytes by the indole-spectrophotometric method as previously described¹. Alternatively two blood drops from a $+/+ nu$ and a $+p nu$ animal were smeared side by side on the same slide, postfixed with methanol and Feulgen stained after 6 min 1 N HCl hydrolysis at $60^\circ C$. DNA measurement was carried out with a Leitz histophotometer on at least fifty nuclei of each individual. Comparison was always made between the $+/+ nu$ and the $+p nu$ sample on the same slide normalizing the result taking the $+/+ nu$ equal to 5.95 pg/diploid cell (this value is the average obtained with the indole method for $+/+ nu$ animals). In general the indole method was utilized with adult individuals from a natural population and the histophotometric method with those reared in the laboratory; individuals 7, 8 and 12 have been analysed by both methods. Standard deviation of the mean is given.

RNA and 4S RNA (Fig. 1b and c). As with the total DNA content, at least a first level of reduction is detectable at about 85% of normal in spite of obvious variability owing to technique and to the small number of animals tested. Lower levels, indistinguishable from each other, tend to approach about 65% of normal.

Saturation levels in Fig. 1a, b and c refer to a fixed amount of DNA; to obtain an estimate of the number of genes, these values must be corrected for the DNA content of diploid cells which, as previously mentioned, is different in $+/+ nu$ and $+p nu$ individuals. For example the 85% saturation level, after correction for 5.25 pg/diploid cell DNA content, becomes about 75% of the 100% saturation level corrected for 5.96 pg/diploid cell DNA content. Lower levels tend to approach, after similar correction, a 50% value. This is just what we expect for a reduction of the redundancy of the three gene families analysed to 50%, 25% and so on, with respect to $+/+ nu$, considering that the $p nu$ condition was carried on only one of the two chromosomal sets.

Our findings lead us to envisage an uninemic eukaryotic chromosome the DNA sequences of which can be distinguished in a backbone along which, at fixed loci, redundant genes are inserted. Non-redundant genes can also be present, as with globin genes^{10,11}. Meiosis, that is the amplification phase, would present the opportunity for both redundancy regulation and genetic rectification of gene families. Although gene families share a common regulatory mechanism for redundancy, the absolute number of genes is not necessarily the same for all of them. Occasional changes of the redundancy regulation would cause all gene families to be numerically reduced to one half, one quarter and so on of the normal redundancy value. All these events might be explained by a process involving excision and reinsertion of redundant genes from the backbone in analogy with the model we proposed for rRNA genes¹. Although a more detailed interpretation of these events is at present impossible, the analogy between redundant genes and episomes is suggestive.

The $p nu$ condition, up to now thought to involve only the nucleolar organizer, involves in effect most if not all families of redundant genes. The size reduction of the nucleolus would be only a phenotypic aspect useful for the selection of these "low-redundancy" individuals.

We are reluctant to consider this condition in terms of mutation of a single redundancy regulator gene; in fact individuals with unequal sized nucleoli are found very frequently not only in natural populations but also among the progeny of normal individuals ($+/+ nu \times +/+ nu$). Moreover in this last case the frequency is variable⁵ from one to another cross: we have observed variability from less than 1% up to, in some cases, 30%.

We give a different interpretation to the anucleolate condition ($O nu$), which should be considered in terms of a deletion of the reintegration site for rRNA genes not affecting the redundancy of other gene families; in fact it has been shown⁸ that these individuals have a normal number of 5S and 4S RNA genes. In agreement with this observation the only one $+O nu$ animal we could find had a normal DNA content per diploid cell.

We thank Mrs V. Auturori-Pezzoli and Mr A. Di Francesco, Mr F. Pasquetti and Mr G. Scotti for technical assistance. P. A. L.-S. is on leave from the Facultad de Medicina, Universidad Central de Venezuela, Caracas.

F. AMALDI
P. A. LAVA-SANCHEZ

Centro degli Acidi Nucleici del CNR,
Istituto di Fisiologia Generale

M. BUONGIORNO-NARDELLI

Istituto di Istologia ed Embriologia,
Facoltà di Scienze,
Università di Roma

Received November 13, 1972.

- ¹ Buongiorno-Nardelli, M., Amaldi, F., and Lava-Sanchez, P. A., *Nature New Biology*, **238**, 134 (1972).
- ² Bostock, C., in *Advances in Cell Biology* (edit. by Prescott, D. M., Goldstein, L., and McConkey, E.), **2**, 153 (Appleton-Century-Crofts, New York, 1971).
- ³ Kedes, L. H., and Birnstiel, M. L., *Nature New Biology*, **230**, 165 (1971).
- ⁴ Callan, H. G., and Lloyd, L., *Phil. Trans. Roy. Soc., B*, **243**, 135 (1960).
- ⁵ Miller, L., and Gurdon, J. B., *Nature*, **227**, 1108 (1970).
- ⁶ Davidson, E. H., and Hough, B. R., *Proc. US Nat. Acad. Sci.*, **63**, 343 (1969).
- ⁷ Buongiorno-Nardelli, M., and Amaldi, F., *Nature*, **225**, 946 (1970).
- ⁸ Amaldi, F., and Buongiorno-Nardelli, M., *Exp. Cell Res.*, **65**, 329 (1971).
- ⁹ Brown, D. D., and Weber, C. S., *J. Mol. Biol.*, **34**, 661 (1968).
- ¹⁰ Bishop, J. O., Pemberton, R., and Baglioni, C., *Nature New Biology*, **235**, 231 (1972).
- ¹¹ Harrison, P. R., Hell, A., Birnie, G. D., and Paul, J., *Nature*, **239**, 219 (1972).

Latimeria chalumnae: Reproduction and Conservation

Schultze¹ and Anthony and Millot² have concluded that the coelacanth, *Latimeria chalumnae*, is oviparous. Because of the general interest surrounding this species and considering the implications which modes of reproduction have on proposals for the conservation of *Latimeria*², we believe that a reconsideration of the reproductive mode in coelacanths is in order.

Schultze's arguments for oviparity in *Latimeria* depend on palaeontological data. Yolk sac larvae of the Carboniferous genus *Rhabdoderma* have been described, strong evidence that this particular coelacanth was oviparous. It was noted that neither fossil nor recent coelacanths possess modifications of the anal or pelvic fins for internal fertilization. Internal fertilization would, of course, be necessary for ovoviviparity or viviparity. Schultze further questioned the interpretation of Watson³ that the occurrence of young *Holophagus* (*Undina*) inside a large specimen was evidence of viviparity in this Jurassic fossil, suggesting instead that this form was cannibalistic. The evidence presented by Anthony and Millot related to the enormous size of eggs removed from a recently-caught mature female *Latimeria* (8.5–9.0 cm diameter). They concluded that such large eggs were inconsistent with viviparity.

We agree that this evidence makes the occurrence of viviparity in *Latimeria* unlikely, but we feel that there is a strong possibility that the living coelacanth is ovoviviparous, providing an ample supply of nourishment prior to fertilization but maintaining the embryo internally during early development. Evidence in favour of ovoviviparity comes from physiological, morphological and palaeontological sources.

Latimeria retains high levels of urea to maintain its blood osmolality near that of seawater^{4,5}. Viviparity, ovoviviparity, or the existence of a cleidoic egg are necessary concomitants of urea retention in Chondrichthyes, the only other major group to use the retention of urea for osmoregulatory purposes^{4,6,7}. There is no evidence that *Latimeria* has a section of the oviduct modified for the production of egg membranes and cases necessary for the deposition of cleidoic eggs². Embryonic elasmobranchs do not develop the capacity for renal and extrarenal retention of urea until late in development⁷ which makes a protected foetal environment necessary. In oviparous forms the egg case and membranes restrict the loss of urea until the embryo is capable of urea production⁸, and ovoviviparous and viviparous species provide a constant embryonic milieu through maternal urea production.

Morphological adaptations to ovoviviparity would necessarily include some means of internal fertilization. *Latimeria* lacks pelvic claspers or an anal fin specialized as an intromittent organ (indeed the role of the latter in locomotion⁹ would seem to preclude such specializations), but males do possess modi-

fications of the cloacal region^{10,11}, which are possibly developments for internal fertilization. These are the extension of the cloaca into a tubercle, and a series of surrounding erectile caruncles. In some teleosts and all tetrapods which have internal fertilization the intromittent organ originates from the cloacal region rather than from fins or limbs.

The palaeontological data are not inconsistent with our hypothesis of ovoviviparity in coelacanths. We feel that Watson's³ arguments in favour of the proposition that young *Holophagus* were undergoing development rather than digestion within the large specimen are convincing although not completely conclusive. Although we admit that Schultze's interpretation that *Rhabdoderma* was oviparous is probably justified, we note that this species was at least partially of freshwater habitat and both *Holophagus* and *Latimeria* seem to have been exclusively marine¹². In elasmobranchs urea retention is a specific adaptation to marine environments that is abandoned in groups such as the family Potamotrygonidae which are restricted to freshwater¹³⁻¹⁵. *Rhabdoderma* probably did not retain high levels of urea and its embryos would not have required a protected environment. (It is possible, but not likely, that *Rhabdoderma* was ovoviviparous and the yolk sac larvae described by Schultze were released prematurely from a dead or dying female.) The genus did possess large eggs (15-16 mm in diameter¹) which could be regarded as either a "preadaptation" to or relic of ovoviviparity. Finally, we note that the very size of *Latimeria* eggs is a strong argument that this species has a high degree of parental care, involving either ovoviviparity or guarding a cluster of eggs. It is difficult to believe that *Latimeria* could survive at all unless the small number of eggs were protected from predation.

Our discussion has pointed to the possibility that *Latimeria* is ovoviviparous. That a question still exists regarding the mode of reproduction in this fish indicates the need for a regular collection of data (presumably by the officials in the Comoro Islands responsible for the preparation and distribution of specimens) on sex, reproductive state, and possible occurrence of embryos of coelacanths caught in the future. Few of the more than 70 caught have been dissected and the sex of many has not been recorded (20 females and 25 males have been reported¹⁶). Another step which might be taken is to check the specimens which have already been caught and sent to various universities and museums for reproductive condition. We understand the reluctance of museums to dissect exhibition specimens, but the surgery involved would involve only a minor extension of the incision routinely made in the Comoro Islands, and the discovery of embryos would greatly enhance both the scientific and popular value of the fish. Fortunately, the officials in the Comores have maintained records of the dates, depths, and sites of capture for each specimen and it should be possible to reconstruct any seasonal or migrational pattern in the reproduction of *Latimeria*.

We share the concern of Anthony and Millot² for the protection of *Latimeria*, but we feel that their proposal for a ban on coelacanth fishing from December 15 to March 15 is premature until more is known of the reproductive biology and population levels of the fish. Male *Latimeria* are seemingly in active spermatogenesis from September until February¹⁷ and large eggs have been reported in January, which suggests that breeding occurs somewhere between November and February. If coelacanths are oviparous and do not guard a cluster of eggs the December to March interdiction proposed by Anthony and Millot might be justified. If, on the other hand, *Latimeria* is ovoviviparous, the time during which the embryos are maintained inside the mother (which could extend for many months as in elasmobranchs) would be the phase most deserving of protection.

We are not, however, fully convinced that the present fishery for coelacanths represents a serious threat to the survival of the species. Aside from the initial specimen from South Africa, all known coelacanths have been caught in the Comoro Islands by native fishermen using the same techniques and

equipment which they have been using for many centuries. The natives do not normally fish specifically for coelacanths but catch them while seeking other species, principally the oilfish, *Ruvettus pretiosus*. The reward offered by the government, while generous, does not provide a sufficient incentive to justify a deliberate fishing effort for *Latimeria*, which is uncommonly caught. Whether the infrequency with which coelacanths appear (some 3-4 per year) is due to rarity, to the inaccessibility of their habitat, or to inefficiency of native fishing is uncertain. From records kept by Comorean officials there seems to be no change in the catch rate of coelacanths caught over the years in which such data were collected. This suggests to us that the population is relatively stable.

The observation that *Latimeria* is relatively common at only a few sites within the Comores² could be due to sampling error rather than a real population distribution. The areas from which most coelacanths have been reported (the west coasts of Grande Comore and Anjouan) include large fishing villages in which a substantial night fishery for *Ruvettus* occurs. Other regions in the Comores may well lack the concentrated fisheries which occur in these areas. Indeed, the restriction of *Ruvettus* longline fisheries to the Comoro, Society and Cook island groups¹⁸ may well be the principal reason that, with the exception of the initial South African record, *Latimeria* is known only from the Comores.

We are not convinced that *Latimeria* is currently in danger of extinction, but we feel that studies to determine the population level of this fish in the Comores are critical for the establishment of a sound policy of conservation which would both protect *Latimeria* from possible overfishing and preserve the livelihood of the native fishermen. Such studies could include a survey of *Latimeria* habitats utilizing submersibles, remote television and photography. The possibility of tagging-recapture studies might also be considered after it has been determined what factors are responsible for the limited ability of *Latimeria* to survive in surface waters^{9,19,20}. If feasible, such studies could provide data on growth, migrational patterns, and population density while harming neither the *Latimeria* population nor the native economy.

R. W. GRIFFITH
K. S. THOMSON

Department of Biology and
Peabody Museum of Natural History,
Yale University,
New Haven, Connecticut

Received December 17, 1972.

- ¹ Schultze, H.-P., *Nature New Biology*, **236**, 90 (1972).
- ² Anthony, J., and Millot, J., *C.R. Acad. Sci. Paris*, **274**, 1925 (1972).
- ³ Watson, D. M. S., *Proc. Zool. Soc. Lond.*, **453** (1927).
- ⁴ Pickford, G. E., and Grant, F. B., *Science*, **155**, 568 (1967).
- ⁵ Lutz, P. L., and Robertson, J. D., *Biol. Bull.*, **141**, 553 (1971).
- ⁶ Smith, H. W., *From Fish to Philosopher* (Little Brown, Boston, 1953).
- ⁷ Price, K. S., and Daiber, F. C., *Physiol. Zool.*, **40**, 248 (1967).
- ⁸ Read, L. J., *Biol. Bull.*, **135**, 537 (1968).
- ⁹ Locket, N. A., and Griffith, R. W., *Nature*, **237**, 175 (1972).
- ¹⁰ Millot, J., and Anthony, J., *Bull. Mus. Nat. Hist. Naturelle*, second ser., **32**, 287 (1960).
- ¹¹ Smith, J. L. B., *Nature*, **171**, 99 (1953).
- ¹² Thomson, K. S., *Biol. Rev.*, **44**, 91 (1969).
- ¹³ Thorson, T. B., Cowan, C. M., and Watson, D. E., *Science*, **158**, 375 (1967).
- ¹⁴ Junqueira, L. C. U., Hoxter, G., and Zago, D., *Rev. Bras. Pesq. Med. Biol.*, **1**, 225 (1968).
- ¹⁵ Griffith, R. W., Pang, P. K. T., Srivastava, A. K., and Pickford, G. E., *Biol. Bull.* (in the press).
- ¹⁶ Millot, J., Anthony, J., and Robineau, D., *Bull. Mus. Nat. Hist. Naturelle*, third ser., **55** (in the press).
- ¹⁷ Millot, J., and Anthony, J., in *Traité de Zoologie* (edit. by Grasse, P.), **13**, 2584 (1958).
- ¹⁸ Forster, G. R., Badcock, J. R., Longbottom, M. R., Merrett, N. R., and Thomson, K. S., *Proc. Roy. Soc. Lond.*, **B**, **175**, 367 (1970).
- ¹⁹ Millot, J., *Nature*, **175**, 362 (1955).
- ²⁰ Smith, J. L. B., *Nature*, **175**, 473 (1955).

The Effect of Drilling Date on the Seed Yield of the Navy Bean (*Phaseolus vulgaris*)

EACH year around 50,000 tons of Navy beans are imported into the UK from the Michigan area of the USA. More than one million cans (as "baked beans") are sold in Britain each day.

Trials carried out in the UK in the 1950s showed that the growing season was too short to allow ripening, but work begun in 1970 by the Pea Growing Research Organization indicated that the two varieties Seafarer and Revenge can be grown successfully in Britain. We undertook a trial in 1972 to investigate the most suitable time to drill one of these varieties.

The variety Seafarer was drilled with a single row Stanhay precision drill in 12 inch rows with 3 inch spacing (429,780 seeds per hectare) on the following dates: first sowing (N_1), May 3; second sowing (N_2), May 17; third sowing (N_3), May 31.

The experiment was sited on a calcareous loam overlying chalk gravel and a randomized block layout with four replicates

(beans and shells) per m^2 . The number of pods per plant increased as sowing was delayed and this increase offset the effect of the higher plant population recorded for N_2 (Table 1) such that pod number per m^2 shown in Table 3 was highest in N_3 . Table 3 also emphasizes that "pod fill" occurred rapidly from mid-August at a time when the actual immature pod number per m^2 was declining.

The yield of seed (ton ha^{-1} at 15% moisture content) was estimated weekly from September 21 (Table 4). The final seed yield recorded for the first sowing date was $1.0 \text{ ton } ha^{-1} \pm 0.33 \text{ ton}$. Moisture content declined slowly and towards the

Table 2 Mean Pod Number per Plant

Date (1972)	N_1	N_2	N_3	$\pm$ S.E. (mean)
July 27	0.5	0.03	—	0.19
August 3	3.3	7.6	—	1.08
August 10	4.8	10.7	6.2	0.89
August 17	7.7	12.5	20.4	1.71
August 24	4.8	8.2	21.5	2.55
August 31	5.3	9.1	17.3	1.29
September 7	5.0	9.0	12.5	0.92

Table 1 Established Populations (Plants $ha^{-1} (\times 10^{-3})$)

	1st sowing N_1	2nd sowing N_2	3rd sowing N_3
Number of established plants	138.5	279.4	257.9
Percentage establishment	32	65	60

was used. Each plot measured 33 foot $\times$ 12 foot. Fertilizer was broadcast onto the seedbed on April 27 to provide 135N: 90P: 45K units per acre and after drilling the pre-emergence herbicide 'Ivotin' (dinoseb acetate and monolinuron) was used.

The established plant populations for each sowing date are shown in Table 1; the figures are means of the plant counts which were taken weekly for the period July 27 to September 7. Establishment was greatly reduced by early sowing. There was a cool summer in 1972, June was the coldest recorded since 1916. With the second and third sowing dates a 75% seedling emergence was recorded seven days after drilling, but field losses reduced the established populations to between 60% and 65% of the seed sown.

The components of seed yield are pod number per plant and seed number per pod. Table 2 shows the mean pod number per plant and Table 3 the pod numbers and their dry weights

end of the sampling period levels were higher than those recorded for the later sown plants. This resulted from a long erratic flowering period, probably caused by lack of competition between plants at low population. As a result plants randomly selected for yield estimation carried both ripe and very immature pods throughout the yield sampling period. The final sowing continued to bulk throughout late September and early October and similar yields were recorded for N_2 and N_3 on the final sampling date. In both cases the moisture content steadily declined but N_2 reached 30% m.c. (the earliest point at which it is thought to be ready for harvest) approximately nine days ahead of N_3 on October 5 (Table 4).

Pod number per m^2 (Table 3) is highest in N_3 , but the seed yields of N_2 and N_3 were the same at the final sampling date (Table 4). The number of beans per pod did not differ significantly ($N_1 = 3.5$, $N_2 = 3.8$, $N_3 = 4.0$; S.E. ± 0.23); this indicates that the potential yield of seed in N_3 was offset by external conditions which became increasingly unfavourable for photosynthesis during the ripening period. Although no records were made of seed dry weights from the various sowing dates, seeds from N_3 were smaller than from N_2 .

From the results of this trial it seems unwise to sow this crop before the middle of May and sowings made after the end of May are likely to result in a loss of potential yield and

Table 3 Pod Number and Weight per m^2

Date (1972)	1st sowing (N_1)			2nd sowing (N_2)			3rd sowing (N_3)		
	Pod No. per m^2	Pod wt. g/ m^2	Pod wt. as % of T.D.M./ m^2	Pod No. per m^2	Pod wt. g/ m^2	Pod wt. as % of T.D.M./ m^2	Pod No. per m^2	Pod wt. g/ m^2	Pod wt. as % of T.D.M./ m^2
August 17	91.3	16.5	26.5	284.0	48.7	28.6	466.3	8.3	5.2
September 7	48.2	35.8	55.6	218.0	174.3	62.2	249.5	140.2	51.1
September 21	48.8	54.1	—	215.0	241.0	—	252.6	187.8	—

T.D.M. = total dry matter.

Table 4 Mean Yield of Seed (15% Moisture) and the Moisture Content of the Seed at Each Harvesting Date

	September 21		September 28		October 5		October 12		October 19	
	Seed yield (ton ha^{-1})	Moisture content (%)	Seed yield (ton ha^{-1})	Moisture content (%)	Seed yield (ton ha^{-1})	Moisture content (%)	Seed yield (ton ha^{-1})	Moisture content (%)	Seed yield (ton ha^{-1})	Moisture content (%)
N_1	0.79	55.0	0.95	51.0	1.16	43.5	1.00	37.7	1.00	26.7
N_2	2.42	54.0	2.68	47.2	2.75	29.0	2.43	27.0	2.80	16.0
N_3	1.63	61.5	2.19	57.7	2.74	45.2	2.49	33.7	2.77	17.5
$\pm$ S.E. (mean)	0.28		0.28		0.31		0.27		0.33	

smaller beans. But the cold summer of 1972 must be considered; possibly the sowing period could be commenced from early May in an average year.

The beans grown at Wye in 1972 were canned in tomato sauce by a processing firm who report that the texture and canning quality of the beans compared favourably with those imported.

The future of the Navy bean as a break crop will ultimately be determined by its financial returns per hectare. Current variable costs (seed, fertilizer and sprays) are £66 per hectare and returns for an average crop yielding 2 ton ha⁻¹ are £100–£120 per ton. This would result in a gross margin of £134–£174 per hectare. The programme of research will be expanded in 1973.

D. H. SCARISBRICK
J. M. WILKES

Wye College,
Ashford,
Kent

Octal Notation for Designating Physiologic Races of Plant Pathogens

HABGOOD¹ proposed a system for naming physiologic races of plant pathogens making use of the characteristics of binary notation, in which the reaction of each host to a given isolate is written down using 0 for resistance and 1 for susceptibility, the differential hosts being arranged in a fixed linear order. The resulting pattern of 0s and 1s is then considered as a binary number and converted to decimal notation. This system has been adopted for physiologic races of *Puccinia striiformis* infecting wheat².

Experience of using this system has revealed two disadvantages: (a) it is often difficult to appreciate the correspondence between the decimal race codes and the patterns of susceptibility on the differential hosts unless one is highly accustomed to converting decimal values to binary notation; (b) it is difficult to recognize similarities and differences among the patterns of the decimally coded races.

Against these it may be argued that those who have to work continually with this binary/decimal system will soon become familiar with the susceptibility patterns on the standard differential hosts corresponding to the decimal race codes, while others who require them less frequently can work the patterns out by the method of repeated subtraction of the decimal values of successively smaller powers of 2, as described by Johnson *et al.*². It may also be argued that the patterns are, of themselves, without significance—they merely indicate different physiologic races. This would not, however, be true if this system of nomenclature were used to designate virulence genotypes as suggested by Habgood¹.

From my experience of octal notation, that is, counting with a radix of 8 instead of 10 (decimal) or 2 (binary), I suggest that octal notation would avoid both these disadvantages. The octal system is widely used in listing machine instruction programs for small computers, where groups of six octal digits are written in place of sixteen to eighteen binary digits. The correspondence between binary, octal and decimal notations is shown in Table 1. This correspondence will be the more readily appreciated if the binary patterns are resolved

into groups of 3 binary digits, starting at the least significant, that is, right, end.

Although individual octal race codes would usually be bigger than in decimal notation, the octal system would use no more digits overall. Both notations can accommodate 9 differential hosts within a 3 digit code, that is, 777 octal or 511 decimal. As in the binary/decimal system, the binary/octal

Table 1 Corresponding Values in Binary, Octal and Decimal Notations

Binary	Octal	Decimal
0	0	0
1	1	1
10	2	2
11	3	3
100	4	4
101	5	5
110	6	6
111	7	7
1 000	10	8
10 000	20	16
100 000	40	32
1 000 000	100	64
10 000 000	200	128
100 000 000	400	256

notation is open-ended, permitting the easy addition of further differentials as they become necessary.

From Table 2 it can be seen that some decimal race codes are easily decoded into the corresponding binary patterns of differential host susceptibilities (for example, 36 decimal) and that the relationships among the patterns for some groups of races are quickly clear, such as 36 and 39 decimal. I would suggest, however, that after comparatively little experience users of the binary/octal system will more rapidly appreciate that race 150 octal differs from race 51 octal in not giving a

Table 2 Examples of Race Codes on the Proposed World Series of Differential Varieties for *Puccinia striiformis* in Binary, Octal and Decimal Notations.

Differential variety	Octal code	Decimal code
G F E D C B A		
1 0 0 1 0 0	44	36
1 0 0 1 1 1	47	39
1 0 1 0 0 1	51	41
1 1 0 1 0 0	150	104

susceptible reaction on differential A, is identical in respect of differentials B and C, and D, E and F, and gives an additional susceptible reaction on differential G, while this is not at all clear from the corresponding decimal codes, 104 and 41 respectively.

J. GILMOUR

Edinburgh School of Agriculture,
West Mains Road,
Edinburgh EH9 3JG

Received February 19, 1973.

¹ Habgood, R. M., *Nature*, **227**, 1268 (1970).

² Johnson, R., Stubbs, R. W., Fuchs, E., and Chamberlain, N. H., *Trans. Br. Mycol. Soc.*, **58**, 475 (1972).

CORRESPONDENCE

Science and Politics

SIR,—The discussions about Tel Aviv, Vietnam, politics and parish of *Nature*, including the latest ones by Drs Simonen (*Nature*, 241, 487; 1973), Lloyd (*Nature*, 242, 213; 1973) and Rognes (*Nature*, 242, 143; 1973), must ultimately deal with the questions of how scientifically advanced nations have used science and technology politically and militarily to repress the peoples of scientifically less developed countries, and to what extent science and scientists in the more developed countries are responsible for that effect.

It is difficult to find any period, since the time of Francis Bacon, when science was not employed for, or benefited by, repression and exploitation, mainly by Europeans (later joined by Japanese) against other peoples of the world, the Vietnam war being just the latest example. It is remarkable that, during the past, such a repression was accepted by the scientific community almost without a stir. If Dr Lloyd talks about the politics of the 1930s, let us remember what state the indigenous people of Asia, Africa and Oceania and the non-European people of America were then in, and that colonial European powers as well as the United States and Japan then benefited themselves from that state, while Germany was excluded from it. Let us also remember that the "political nemesis of 1939" was precipitated by a culmination of the desperate struggle, both political and economic, among these nations for more benefit in this sense. Certainly Scandinavians were the least involved.

It is, therefore, really remarkable that through Vietnam scientific communities (mostly guided by younger members and students) have finally come to realize this stark reality and have begun to understand the role played by science during the last three centuries. Insofar as the discussion leads to a revelation of this point, the political arguments put forward by the previous correspondents were all in the thick of *Nature's* parish.

In this sense, preaching may be just as important as any other means to achieve a gradual liberation of scientists from the previously held delusions, at least for some time to come. If scientists all over the world realized that their competition for excellence within the rule of "objective" scientific enterprise is instrumental in the political and military control exercised on "subjective"

criteria, and if they could unite world-over to refuse lending their support to the military end, scientists could exert an influence on politics because politics today cannot operate without scientists. The effect of such actions would be significant only if they were staged simultaneously all over the world. To begin with, scientists in scientifically more advanced nations should learn to direct their attention to those who would suffer rather than benefit from science. However, as little research funding would come from such suffering people, it follows that little scientific research into these problems would be practised for some time. What else other than preaching, based on theories, is effective for even slowly breaking this dilemma?

Lastly, could we not count the emergence of a large number of independent non-European nations all over the world during the last 25 years as examples of successful political preaching?

Yours faithfully,

ATUHIRO SIBATANI

30 Owen Street,
Lindfield, NSW 2070,
Australia

Research Priorities

SIR,—Your copy of January 19 has just become available here. I was deeply dismayed to learn from your editorial (*Nature*, 241, 153; 1973) of the many people (apart from astrophysicists) who are leading lives of discomfort for lack of a "better understanding of the difference between pulsating stars and other kinds of stars". Here, too, we have many people who are leading lives of discomfort, due to such things as malnutrition and malaria, bilharzia and river blindness. Research, such as a deepening of our understanding of the ecology of the vectors of disease, would probably fall into your category of trivial problems. Both these considerations now make it clear to me why, although it is possible to spend many millions of dollars or pounds upon research in radio astronomy, requests for far more modest assistance to develop research schools in Africa upon the trivial problems falls largely upon deaf ears.

It is my sincere hope that the astronomers will soon solve this problem about pulsating stars and other kinds of stars, not only so that the people of the United Kingdom and the United

States "will find the Universe a more comfortable place in which to live", but also because it might then be possible to spare some research funds in the interests of people with a narrow vista and who would be content if we could make just the world a more comfortable place for them.

Yours faithfully,

D. W. EWER

Department of Zoology,
University of Ghana,
PO Box 67, Legon,
Ghana

Psychiatric Decisions

SIR,—Szasz (*Nature*, 242, 305; 1973) continues to raise very important issues demonstrating the arbitrary nature of many psychiatric decisions, and the power society invests in psychiatry. He also raises anxieties, however, by his dogmatic response to imponderable questions.

"Ill", for example, is a word meaning different things in different sentences. It is not necessarily stretching it too far, for example, when describing a woman, aged 26, who after the birth of her child seems intent on suicide and slaughter. Yet Szasz seems to imply that she should be allowed to kill herself without medical intervention, and for killing the child she should go to prison *via* the courts.

I personally believe that many philosophical problems about freedom, illness, and responsibility are outrageously neglected by naive practical psychiatrists. Like many physicians, psychologists and scientists, they are for common sense, and spurn philosophizing as irrelevant hot air. Indeed its only advantage is to show how like an artist the complete physician must be. He must make decisions while thinking in a necessarily inadequate language.

The total impact of what a psychiatrist does is what is important. Szasz rightly challenges a simple view, but in remaining human and avoiding where possible coercion, occasionally a psychiatrist is being a better physician by having the courage to use compulsion in the interest of his patient.

Yours faithfully,

F. A. JENNER

University Department of Psychiatry,
Whiteley Wood Clinic,
Sheffield S10 3TL

Announcements

University News

Professor Jack Tizard, University of London, Institute of Education, has been awarded the Research Award of the American Association on Mental Deficiency for 1973.

Miscellaneous

Dr G. F. Vaughan, Reading, has been appointed to the Medical Research Council as successor to Mr Laurence Pavitt.

Erratum

In the article "Rapid Determination of Very Low Nitrogen Levels in Water" by S. F. Bankert, S. D. Bloom and F. S. Dietrich (*Nature*, 242, 270; 1973) the expression p.p.b. was incorrectly converted during editing to 10^{-9} g kg⁻¹ and should read 10^{-6} g kg⁻¹ throughout.

International Meetings

May 4-5, **Sargassum Meetings** (Dr E. B. G. Jones, Department of Biological Sciences, King Henry I Street, Portsmouth Polytechnic, Portsmouth).

May 15, **The Results of the Spacecraft Missions to Mars** (Executive Secretary, The Royal Society, 6 Carlton House Terrace, London SW1).

May 17, **On Buoyancy and the Lives of Modern and Fossil Cephalopods** (Executive Secretary, The Royal Society, 6 Carlton House Terrace, London SW1).

May 24-25, **Structure and Function of Biological Membranes and Envelopes** (Executive Secretary, The Royal Society, 6 Carlton House Terrace, London SW1).

May 30-31, **Symposium on Field Instru-**

mentation (Secretariat, IEE Conference Department, Savoy Place, London WC2). May 31, **The Value of Automation in the Health Service** (Executive Secretary, The Royal Society, 6 Carlton House Terrace, London SW1).

June 3-8, **2nd Congress—International Society of Aesthetic Plastic Surgery** (The Secretariat, PO Box 16271, Tel Aviv, Israel).

June 4-7, **1st International Symposium on Environmental Mutagens** (Dr B. J. Kilbey, Department of Genetics, University of Edinburgh, Scotland).

June 4-7, **24th Annual Meeting of the Tissue Culture Association** (Local Committee Chairman, Dr George Yerganian, Children's Cancer Research Foundation, 35 Binney Street, Boston, Massachusetts 02115, USA).

June 4-8, **Radioimmunoassay and Related Procedures in Clinical Medicine Research** (Dr E. J. Garcia, International Atomic Energy Agency, Kärtner Ring 11-13, A1010 Vienna, Austria).

June 4-8, **International Marine and Shipping Conference** (The Institute of Marine Engineers, 76 Mark Lane, London EC3R 7JN).

June 4-8, **5th Congress of the International Ergonomics Association** (Organisatie Bureau Amsterdam NV, Postbus 7205, Amsterdam, The Netherlands).

June 4-8, **2nd World Congress on Ultrasonics in Medicine** (Secretariat, 2nd World Congress on Ultrasonics in Medicine, Holland Organizing Centre, 16 Lange Voorhout, The Hague, The Netherlands).

June 4-8, **The Treatment of Malignant Lymphomas** (Miss Lucia Manfredi, Ufficio Attività Didattiche, Istituto Nazionale per lo Studio e la dei Tumori, Via G Venezian, 20133 Milano, Italy).

June 7-9, **International Congress on Im-**

munology (Professor N. Carretti, Obstetrics and Gynaecology Clinic, University of Padua, Via Giustiniani No. 3, Padua 35100, Italy).

June 11, **Charge Transfer Devices** (Meetings Officer, The Institute of Physics, 47 Belgrave Square, London SW1X 8QX).

June 11-13, **Symposium on the Loch Leven IBP Project** (Dr D. S. McLusky, IBP Loch Leven Symposium, Department of Biology, The University, Stirling FK9 4LA).

June 12, **Data for Scientists and Technologists** (Honorary Secretary, Mr D. F. Styles, 55 Penrhyn Avenue, Middleton, Manchester M24 1FP).

June 12-14, **27th Annual Frequency Control Symposium** (Commander, US Army Electronic Command, Dr J. R. Vig, Fort Monmouth, New Jersey 07703, USA).

June 13, **Techniques for the Investigation of Metal Failures** (Meetings Secretary, The Institution of Metallurgists, Northway House, High Road, Whetstone, London N20 9LW).

June 13, **Symposium on Mathematics in Management** (Secretary and Registrar, The Institute of Mathematics, Maitland House, Warrior Square, Southend-on-Sea, Essex SS1 2JY).

June 13-15, **International Conference on Hydrostatic Extrusion** (Mr I. A. McNish, Department of Trade and Industry, Birniehill Institute, National Engineering Laboratory, East Kilbride, Glasgow).

June 13-15, **28th Annual Calorimetry Conference** (James J. Christensen, Department of Chemical Engineering, 138 FELB, Brigham Young University, Provo, Utah 84601, USA).

June 17-21, **18th Annual Meeting of the Health Physics Society** (Mr S. C. Bushong, Department of Radiology, Baylor College of Medicine, Houston, Texas 77025, USA).

HOW TO BUY NATURE

The cost of one year's subscription to NATURE is:

	UK & elsewhere	US & Canada
Nature (Friday)	£16	\$48
Nature & Nature Physical Science or Nature New Biology	£24	\$83
Nature New Biology or Nature Physical Science	£10	\$35
All three editions	£29.50	\$108

(Charges for delivery by air mail on application). Subscribers in North America may be able to claim a tax rebate against their NATURE subscription.

Editorial, Advertising and Publishing Offices of NATURE

MACMILLAN JOURNALS LIMITED
4 LITTLE ESSEX STREET, LONDON WC2R 3LF
Telephone Number: 01-836 6633. Telegrams: Phisus London WC2R 3LF
Telex 262024

MACMILLAN JOURNALS LIMITED
711 NATIONAL PRESS BUILDING
WASHINGTON DC 20004
Telephone Number: 202-737 2355. Telex 64280

International Advertisement Manager

PETER R. KAVANAGH
MACMILLAN JOURNALS LIMITED
4 LITTLE ESSEX STREET, LONDON WC2R 3LF
Telephone Numbers: UK 01-836 6633; USA 202-737 2355

Subscription Department

MACMILLAN JOURNALS LIMITED
BRUNEL ROAD, BASINGSTOKE, HANTS RG21 2XS
Telephone Number: Basingstoke 29242

Classified advertisements

T. G. SCOTT & SON, LIMITED
1 CLEMENT'S INN, LONDON WC2A 2ED
Telephone Number: 01-242 6264/01-405 4743
Telegrams: Textualist London WC2A 2ED

Registered as a newspaper at the Post Office

Copyright © Macmillan Journals Limited, April 27 1973